

British Association for the Advancement of Science.

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ADDRESS

BY

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PRESIDENT.

THE late Lord Iddesleigh delighted an audience, for a whole evening, by an address on 'Nothing.' Would that I had his talents, and could discourse to you as charmingly as he did to his audience, but I dare not try to talk about 'Nothing.' I do however propose, as one of the two sections of my Address, to discourse to you on the importance of the 'Next-to-Nothing.' The other section is far removed from this microscopic quantity, as it will embrace the 'Eulogy of the Civil Engineer and will point out the value to science of his works.'

I do not intend to follow any system in dealing with these two sections. I shall not even do as Mr. Dick, in 'David Copperfield,' did—have two papers, to one of which it was suggested he should confine his Memorial and his observations as to King Charles's head. The result is, you will find, that the importance of the next-to-nothing, and the laudation of the Civil Engineer, will be mixed up in the most illogical and haphazard way, throughout my Address. I will leave to such of you as are of orderly minds, the task of rearranging the subjects as you see fit, but I trust—arrangement or no arrangement—that by the time I have brought my Address to a conclusion, I shall have convinced you that there is no man who more thoroughly appreciates the high importance of the 'next-to-nothing,' than the Civil Engineer of the present day, the object of my eulogy this evening.

If I may be allowed to express the scheme of this Address in modern musical language, I will say that the 'next-to-nothing' 'motive' will commonly usher in the 'praise-song' of the Civil Engineer; and it seems to me will do this very fitly, for in many cases it is by the patient and discriminating attention paid to the effect of the 'next-to-nothing' that

the Civil Engineer of the present day has achieved some of the labours of which I now wish to speak to you.

An Association for the Advancement of Science is necessarily one of such broad scope in its objects, and is so thoroughly catholic as regards Science, that the only possible way in which it can carry out those objects at all, is to segregate its members into various subsidiary bodies, or sections, engaged on particular branches of Science. Even when this division is resorted to, it is a hardy thing to say that every conceivable scientific subject can be dealt with by the eight Sections of the British Association. Nevertheless, as we know, for fifty-seven years the Association has carried on its labours under Sections, and has earned the right to say that it has done good service to all branches of Science.

Composed, as the Association is, of a union of separate Sections, it is only right and according to the fitness of things that, as time goes on, your Presidents should be selected, in some sort of rotation, from the various Sections. This year it was felt, by the Council and the Members, that the time had once more arrived when Section G—the Mechanical Section—might put forward its claim to be represented in the Presidency; the last time on which a purely engineering Member filled the chair having been at Bristol in 1875, when that position was occupied by Sir John Hawkshaw. It is true that at Southampton, in 1882, our lamented friend, Sir William Siemens, was President, and it is also true that he was a most thorough engineer and representative of Section G; but all who knew his great scientific attainments will probably agree that, on that occasion, it was rather the Physical Section A which was represented, than the Mechanical Section G.

I am aware, it is said, Section G does not contribute much to pure Science by original research, but that it devotes itself more to the application of Science. There may be some foundation for this assertion, but I cannot refrain from the observation, that when Engineers, such as Siemens, Rankine, Sir William Thomson, Fairbairn, or Armstrong, make a scientific discovery, Section A says it is made, not in the capacity of an Engineer, and, therefore, does not appertain to Section G, but in the capacity of a Physicist, and therefore appertains to Section A—an illustration of the danger of a man's filling two positions, of which the composite Prince-Bishop is the well-known type. But I am not careful to labour this point, or even to dispute that Section G does not do much for *original* research. I don't agree it is a fact, but, for the purposes of this evening, I will concede it to be so. But what then? This Association is for the 'Advancement of Science'—the *Advancement* be it remembered; and I wish to point out to you, and I trust I shall succeed in establishing, that for the *Advancement* of Science it is absolutely necessary there should be the *Application* of Science, and that, therefore, the Section, which as much as any other (or, to state the fact more truly, which more than any other) in the Association *applies* Science, is doing

a very large share of the work of *advancing* Science, and is fully entitled to be periodically represented in the Presidency of the whole Association.

I trust also I shall prove to you that applications of Science, and discoveries in pure Science, act and re-act the one upon the other. I hope in this to carry the bulk of my audience with me, although there are some, I know, whose feelings, from a false notion of respect for Science, would probably find vent in the 'toast' which one has heard in another place—this 'toast' being attributed to the Pure Scientist—'Here's to the latest scientific discovery: may it never do any good to anybody!'

To give an early illustration of this action and re-action, which I contend occurs: take the well-worn story of Galileo, Torricelli, and the pump-maker. It is recorded that Galileo first, and his pupil Torricelli afterwards, were led to investigate the question of atmospheric pressure, by observing the failure of a pump to raise water by 'suction,' above a certain level. Perhaps you will say the pump-maker was not applying science, but was working without science. I answer, he was unknowingly applying it, and it was from that which arose in this unconscious application that the mind of the Pure Scientist was led to investigate the subject, and thereupon to discover the primary fact, of the pressure of the atmosphere, and the subsidiary facts which attend thereon. It may appear to many of you that the question of the exercise of pressure by the atmosphere should have been so very obvious, but little merit ought to have accrued to the discoverer; and that the statement, once made, must have been accepted almost as a mere truism. This was, however, by no means the case. Sir Kenelm Digby, in his 'Treatise on the Nature of Bodies,' printed in 1658, disputes the proposition altogether, and says, in effect, he is quite sure, the failure of the pump to raise water was due to imperfect workmanship of some kind or description, and had nothing to do with the pressure of the air; and that there is no reason why a pump should not suck up water to any height. He cites the boy's sucker, which, when applied to a smooth stone, will lift it, and he says the reason why the stone follows the sucker is this. Each body must have some other body in contact with it. Now, the stone being in contact with the sucker, there is no reason why that contact should be broken up, for the mere purpose of substituting the contact of another body, such as the air. It seems pretty clear, therefore, that even to an acute and well-trained mind, such as that of Sir Kenelm Digby, it was by no means a truism, and to be forthwith accepted when once stated, that the rise of water on the 'suction side' of a pump was due to atmospheric pressure. I hardly need point out that the pump-maker should have been a member of 'G.' Galileo and Torricelli, led to reflect by what they saw, should have been members of 'A' of the then 'Association for the Advancement of Science.'

But, passing away from the question of the value of the application

of Science of a date some two and a half centuries ago, let us come a little nearer to our own times.

Electricity—known in its simplest form to the Greeks by the results arising from the friction on amber, and named therefrom; afterwards produced from glass cylinder machines, or from plate machines; and produced a century ago by the ‘Influence’ machine—remained, as did the discoveries of Volta and Galvani, the pursuit of but a few, and even the brilliant experiments of Davy did not suffice to give very great impetus to this branch of physical science.

Ronalds, in 1823, constructed an electric telegraph. In 1837 the first commercial use was made of the telegraph, and from that time electrical science received an impulse such as it had never before experienced. Further scientific facts were discovered; fresh applications were made of these discoveries. These fresh applications led to renewed vigour in research, and there was the action and reaction of which I have spoken. In the year 1871 the Society of Telegraph Engineers was established. In the year 1861 our own Association had appointed a Committee to settle the question of electrical standards of resistance, which Committee, with enlarged functions, continued its labours for twenty years, and of this Committee I had the honour of being a member. The results of the labours of that Committee endure (somewhat modified, it is true), and may be pointed to as one of the evidences of the value of the work done by the British Association. Since Ronalds’s time, how vast are the advances which have been made in electrical communication of intelligence, by land lines, by submarine cables all over the world, and by the telephone! Few will be prepared to deny the statement, that pure electrical science has received an enormous impulse, and has been advanced by the commercial application of electricity to the foregoing, and to purposes of lighting. Since this latter application, scores, I may say hundreds, of acute minds have been devoted to electrical science, stimulated thereto by the possibilities and probabilities of this application.

In this country, no doubt, still more would have been done if the lighting of districts from a central source of electricity had not been, since 1882, practically forbidden by the Act passed in that year. This Act had in its title the facetious statement that it was ‘to facilitate Electrical Lighting’—although it is an Act which, even modified as it has been this year, is still a great discouragement of free enterprise, and a bar to progress. The other day a member of the House of Commons was saying to me: ‘I think it is very much to our discredit in England that we should have allowed ourselves to be outrun in the distribution of electric lighting to houses, by the inhabitants of the United States, and by those of other countries.’ Looking upon him as being one of the authors of the ‘facetious’ Act, I thought it pertinent to quote the case of the French parricide, who, being asked what he has to say in mitigation of punishment, pleads, ‘Pity a poor orphan’—

the parricide and the legislator being both of them authors of conditions of things which they affect to deplore. I will say no more on this subject, for I feel that it would not be right to take advantage of my position here to-night to urge Political Economy views, which should be reserved for Section F. I will merely, and as illustrative of my views of the value of the application of Science to Science itself, say there is no branch of physics pursued with more zeal and with more happy results than that of electricity, with its allies, and there is no branch of Science towards which the public looks with greater hope of practical benefits; a hope that, I doubt not, will be strengthened after we have had the advantage of hearing one of the ablest followers of that science, Professor Ayrton, who, on Friday next, has been good enough to promise to discourse on 'The Electrical Transmission of Power.'

One of the subjects which, as much as (or probably more than) any other, occupies the attention of the engineer, and therefore of Section G, is that of (the so-called) Prime Movers, and I will say boldly that, since the introduction of printing by the use of movable type, nothing has done so much for civilisation as the development of these machines. Let us consider these prime movers—and, first, in the comparatively humble function of replacing that labour which might be performed by the muscular exertion of human beings, a function which at one time was looked upon by many kindly but short-sighted men as taking the bread out of the mouth of the labourer (as it was called), and as being therefore undesirable. I remember revisiting my old schoolmaster, and his saying to me, shaking his head: 'So you have gone the way I always feared you would, and are making things of iron and brass, to do the work of men's hands.'

It must be agreed that all honest and useful labour is honourable, but when that labour can be carried out without the exercise of any intelligence, one cannot help feeling that the result is likely to be intellectually lowering. Thus it is a sorry thing to see unintelligent labour, even although that labour be useful. It is but one remove from unintelligent labour which is not useful; that kind of labour generally appointed (by means of the tread-wheel or the crank) as a punishment for crime. Consider even the honourable labour (for it is useful, and it is honest) of the man who earns his livelihood by turning the handle of a crane, and compare this with the labour of a smith, who, while probably developing more energy by the use of his muscles, than is developed by the man turning the crane-handle, exercises at the same time the powers of judgment, of eye, and of hand in a manner which I never see without my admiration being excited. I say that the introduction of prime movers as a mere substitute for unintelligent manual labour is in itself a great aid to civilisation and to the raising of humanity, by rendering it very difficult, if not impossible, for a human being to obtain a livelihood by unintelligent work—the work of the horse in the mill, or of the turnspit.

But there are prime movers and prime movers—those of small dimensions, and employed for purposes where animal power or human power might be substituted, and those which attain ends that by no conceivable possibility could be attained at all by the exertion of muscular power.

Compare a galley, a vessel propelled by oars, with the modern Atlantic liner; and first let us assume that prime movers are non-existent and that this vessel is to be propelled galley-fashion. Take her length as some 600 feet, and assume that place be found for as many as 400 oars on each side, each oar worked by three men, or 2,400 men; and allow that six men under these conditions could develop work equal to one horse-power: we should have 400 horse-power. Double the number of men, and we should have 800 horse-power, with 4,800 men at work, and at least the same number in reserve, if the journey is to be carried on continuously. Contrast the puny result thus obtained with the 19,500 horse-power given forth by a large prime mover of the present day, such a power requiring, on the above mode of calculation, 117,000 men at work and 117,000 in reserve; and these to be carried in a vessel less than 600 feet in length. Even if it were possible to carry this number of men in such a vessel, by no conceivable means could their power be utilised so as to impart to it a speed of twenty knots an hour, weighing as it would some 10,500 tons gross.

This illustrates how a prime mover may not only be a mere substitute for muscular work, but may afford the means of attaining an end, that could not by any possibility be attained by muscular exertion, no matter what money was expended or what galley-slave suffering was inflicted.

Take again the case of a railway locomotive. From 400 to 600 horse-power developed in an implement which, even including its tender, does not occupy an area of more than fifty square yards, and that draws us at sixty miles an hour. Here again, the prime mover succeeds in doing that which no expenditure of money or of life could enable us to obtain from muscular effort.

To what, and to whom, are these meritorious prime movers due? I answer: to the application of science, and to the labours of the civil engineer, using that term in its full and proper sense, as embracing all engineering other than military. I am, as you know, a Civil Engineer, and I desire to laud my profession and to magnify mine office; and I know of no better means of doing this than by quoting to you the definition of 'civil engineering,' given in the Charter of The Institution of Civil Engineers, namely, that it is 'the art of directing the great sources of power in Nature for the use and convenience of man.' These words are taken from a definition or description of engineering given by one of our earliest scientific writers on the subject, Thomas Tredgold, who commences that description by the words above quoted, and who, having given

various illustrations of the civil engineer's pursuits, introduces this pregnant sentence :—

‘This is, however, only a brief sketch of the objects of civil engineering, the real extent to which it may be applied is limited only by the progress of science; its scope and utility will be increased with every discovery in philosophy, and its resources with every invention in mechanical or chemical art, since its bounds are unlimited, and equally so must be the researches of its professors.’

‘The art of directing the great sources of power in Nature for the use and convenience of man.’ Among all secular pursuits, can there be imagined one more vast in its scope, more beneficent, and therefore more honourable, than this? There are those, I know—hundreds, thousands—who say that such pursuits are not to be named as on a par with those of literature; that there is nothing ennobling in them; nothing elevating; that they are of the earth, earthy; are mechanical, and are unintellectual, and that even the mere bookworm, who, content with storing his own mind, neither distributes those stores to others nor himself originates, is more worthily occupied than is the civil engineer.

I deny this altogether, and, while acknowledging, with gratitude, that, in literature, the masterpieces of master minds have afforded, and will afford, instruction, delight, and solace for all generations, so long as civilisation endures, I say that the pursuits of civil engineering are worthy of occupying the highest intelligence, and that they are elevating and ennobling in their character.

Remember the sapient words of Sir Thomas Browne, who said, when commenting on the want of usefulness of the mere bookworm, ‘I make not, therefore, my head a grave, but a treasure of knowledge, and study not for mine own sake only, but for those who study not for themselves.’ The engineer of the present day finds that he must not make his ‘head a grave,’ but that, if he wishes to succeed, he must have, and must exercise, scientific knowledge; and he realises daily the truth that those who are to come after him must be trained in science, so that they may readily appreciate the full value of each scientific discovery as it is made. Thus the application of science by the engineer not only stimulates those who pursue science, but adds him to their number.

Holding, as I have said I do, the view that he who displaces unintelligent labour is doing good to mankind, I claim for the unknown engineer who, in Pontus, established the first water-wheel of which we have a record, and for the equally unknown engineer who first made use of wind for a motor, the title of pioneers in the raising of the dignity of labour, by compelling the change from the non-intelligent to the intelligent.

With respect to these motors—wind and water—we have two proverbs which discredit them: ‘Fickle as the wind,’ ‘Unstable as water.’

Something more trustworthy was needed—something that we were sure of having under our hands at all times. As a result, Science was

applied, and the 'fire' engine, as it was first called, the 'steam' engine, as it was re-named, a form of 'heat' engine, as we now know it to be, was invented.

Think of the early days of the steam-engine—the pre-Watt days. The days of Papin, Savory, Newcomen, Smeaton! Great effects were produced, no doubt, as compared with no fire engine at all; effects so very marked as to extort from the French writer, Belidor, the tribute of admiration he paid to the 'fire' engine erected at the Fresnes Colliery by English engineers. A similar engine worked the pumps in York Place (now the Adelphi) for the supply of water to portions of London. We have in his work one of the very clearest accounts, illustrated by the best engravings (absolute working drawings), of the engine which had excited his admiration. These drawings show the open-topped cylinder, with condensation taking place below the piston, but with the valves worked automatically.

It need hardly be said that, noteworthy as such a machine was, as compared with animal power, or with wind or water motors, it was of necessity a most wasteful instrument as regards fuel. It is difficult to conceive in these days how, for years, it could have been endured that at each stroke of the engine the chamber that was to receive the steam at the next stroke was carefully cooled down beforehand by a water injection.

Watt, as we know, was the first to perceive, or, at all events, to cure, this fundamental error which existed prior to his time in the 'fire' engine. To him we owe condensation in a separate vessel, the doing away with the open-topped cylinder, and the making the engine double-acting; the parallel motion; the governor; and the engine indicator, by which we have depicted for us the way in which the work is being performed within the cylinder. To Watt, also, we owe that great source of economic working—the knowledge of the expansive force of steam; and to his prescience we owe the steam jacket, without which expansion, beyond certain limits, is practically worthless. I have said 'prescience'—foreknowledge—but I feel inclined to say that, in this case, prescience may be rendered 'pre-Science,' for I think that Watt *felt* the utility of the steam jacket, without being able to say on what ground that utility was

I have already spoken in laudatory terms of Tredgold, as being one of the earliest of our scientific engineering writers, but, as regards the question of steam jacketing, Watt's prescience was better than Tredgold's science, for the latter condemns the steam jacket, as being a means whereby the cooling surfaces are enlarged, and whereby, therefore, the condensation is increased.

I think it is not too much to say, that engineers who, since Watt's days, have produced machines of such marvellous power—and, compared with the engines of Watt's days, of so great economy—have, so far as principles are concerned, gone upon those laid down by Watt. Details

of the most necessary character—necessary to enable those principles to be carried out—have, indeed, been devised since the days of Watt. Although it is still a very sad confession to have to make, that the very best of our steam engines only utilises about one-sixth of the work which resides (if the term may be used) in the fuel that is consumed, it is, nevertheless, a satisfaction to know that great economical progress has been made, and that the 6 or 7 lbs. of fuel per horse-power per hour consumed by the very best engines of Watt's days, when working with the aid of condensation, is now brought down to about one-fourth of this consumption; and this in portable engines, for agricultural purposes, working without condensation—engines of small size, developing only 20 horse-power; in such engines the consumption has been reduced to as little as 1·85 lb. per brake horse-power per hour, equal to 1·65 lb. per indicated horse-power per hour, as was shown by the trials at the Royal Agricultural Society's meeting at Newcastle last year—trials in which I had the pleasure of participating.

In these trials, Mr. William Anderson, one of the Vice-Presidents of Section G, and I were associated, and, in making our report of the results, we adopted the balance-sheet system, which I suggested and used so long ago as 1873 (see vol. 52, pages 154 and 155, of the 'Minutes of Proceedings of the Institution of Civil Engineers'), and to which I alluded in my address as President of G at Montreal.

I have told you that the engineer of the present day appreciates the value of the 'next-to-nothings.' There is an old housekeeping proverb that, if you take care of the farthings and the pence, the shillings and the pounds will take care of themselves. Without the balance-sheet one knows that for the combustion of 1 lb. of coal, the turning into steam of a given quantity of water at a given pressure is obtained. It is seen, at once, that the result is much below that which should be had, but to account for the deficiency is the difficulty. The balance-sheet, dealing with the most minute sources of loss—the farthings and the pence of economic working—brings you face to face with these, and you find that improvement must be sought in paying attention to the 'next-to-nothings.'

Just one illustration. The balance-sheet will enable you at a glance to answer this among many important questions. Has the fuel been properly burnt?—with neither too much air, nor too little.

At the Newcastle trials our knowledge as to whether we had the right amount of air for perfect combustion was got by an analysis of the waste gases, taken continuously throughout the whole number of hours' run of each engine, affording, therefore, a fair average. The analysis of any required portion of gases thus obtained was made in a quarter of an hour's time by the aid of the admirable apparatus invented by Mr. Stead, and, on the occasion to which I refer, manipulated by him. In one instance an excess of air had been supplied, causing a percentage

of loss of 6·34. In the instance of another engine there was a deficiency of air, resulting in the production of carbonic oxide, involving a loss of 4 per cent. The various percentages of loss, of which each one seems somewhat unimportant, in the aggregate amounted to 28 per cent., and this with one of the best boilers. This is an admirable instance of the need of attention to apparently small things.

I have already said that we now know the steam engine is really a heat engine. At the York Meeting of our Association I ventured to predict that, unless some substantive improvement were made in the steam engine (of which improvement, as yet, we have no notion), I believed its days, for small powers, were numbered, and that those who attended the centenary of the British Association in 1931 would see the present steam engines in museums, treated as things to be respected, and of antiquarian interest to the engineers of those days, such as are the open-topped steam cylinders of Newcomen and of Smeaton to ourselves. I must say I see no reason, after the seven years which have elapsed since the York Meeting, to regret having made that prophecy, or to desire to withdraw it.

The working of heat engines, without the intervention of the vapour of water, by the combustion of the gases arising from coal, or from coal and from water, is now not merely an established fact, but a recognised and undoubted commercially economical means of obtaining motive power. Such engines, developing from 1 to 40-horse-power, and worked by the ordinary gas supplied by the gas mains, are in most extensive use in printing works, hotels, clubs, theatres, and even in large private houses, for the working of dynamos to supply electric light. Such engines are also in use in factories, being sometimes driven by the gas obtained from 'culm' and steam, and are giving forth a horse-power for, it is stated, as small a consumption as one pound of fuel per hour.

It is hardly necessary to remind you—but let me do it—that, although the saving of half a pound of fuel per horse-power appears to be insignificant, when stated in that bald way, one realises that it is of the highest importance when that half-pound turns out to be 33 per cent. of the whole previous consumption of one of those economical engines to which I have referred.

The gas engine is no new thing. As long ago as 1807, a M. de Rivaz proposed its use for driving a carriage on ordinary roads. For anything I know he may not have been the first proposer. It need hardly be said that in those days he had not illuminating gas to resort to, and he proposed to employ hydrogen. A few years later, a writer in 'Nicholson's Journal,' in an article on 'flying machines,' having given the correct statement that all that is needed to make a successful machine of this description is to find a sufficiently light motor, suggests that the direction in which this may be sought is the employment of illuminating gas, to operate by its explosion on the piston of an engine. The idea of the gas engine

was revived, and formed the subject of a patent by Barnett in the year 1838. It is true this gentleman did not know very much about the subject, and that he suggested many things which, if carried out, would have resulted in the production of an engine which could not have worked ; but he had an alternative proposition which would have worked.

Again, in the year 1861, the matter was revived by Lenoir, and in the year 1865, by Hugon, both French inventors. Their engines obtained some considerable amount of success and notoriety, and many of them were made and used ; but in the majority of cases they were discarded as wasteful and uncertain. The Institution of Civil Engineers, for example, erected a Lenoir in the year 1868, to work the ventilating fan, but after a short time they were compelled to discard it and to substitute an hydraulic engine.

At the present time, as I have said, gas engines are a great commercial success, and they have become so by the attention given to small things, in popular estimation—to important things, in fact, with which, however, I must not trouble you. Messrs. Crossley Brothers, who have done so much to make the gas engine the commercial success that it is, inform me that they are prosecuting improvements in the direction of attention to detail, from which they are obtaining greatly improved results.

But, looking at the wonderful petroleum industry, and at the multifarious products which are obtained from the crude material, is it too much to say, that there is a future for motor engines, worked by the vapour of some of the more highly volatile of these products—true vapour—not a gas, but a condensable body, capable of being worked over and over again ? Numbers of such engines, some of as much as 4 horse-power, made by Mr. Yarrow, are now running, and are apparently giving good results ; certainly excellent results as regards the compactness and lightness of the machinery ; for boat purposes they possess the great advantage of being rapidly under way. I have seen one go to work within two minutes of the striking of the match to light the burner.

Again, as we know, the vapour of this material has been used as a gas in gas engines, the motive power having been obtained by direct combustion.

Having regard to these considerations, was I wrong in predicting that the heat engine of the future will probably be one independent of the vapour of water ? And, further, in these days of electrical advancement, is it too much to hope for the direct production of electricity from the combustion of fuel ?

As the world has become familiar with prime movers, the desire for their employment has increased. Many a householder could find useful occupation for a prime mover of $\frac{1}{4}$ or $\frac{1}{2}$ horse-power, working one or two hours a day ; but the economical establishment of a steam engine is not possible until houses of very large dimensions are reached, where

space exists for the engine, and where, having regard to the amount of work to be done, the incidental expenses can be borne. Where this cannot be, either the prime mover, with the advantages of its use, must be given up as a thing to be wished for, but not to be procured, or recourse must be had to some other contrivance—say to the laying on of power, in some form or another, from a central source.

I have already incidentally touched upon one mode of doing this, namely, the employment of illuminating gas, as the working agent in the gas engine; but there are various other modes, possessing their respective merits and demerits—all ingenious, all involving science in their application, and all more or less in practical use—such as the laying-on of special high-pressure water, as is now being extensively practised in London, in Hull, and elsewhere. Water at 700 lbs. pressure per inch is a most convenient mode of laying on a large amount of power, through comparatively small pipes. Like electricity, where, when a high electromotive force is used, a large amount of energy may be sent through a small conductor, so with water, under high pressure, the mains may be kept of reasonable diameters, without rendering them too small to transmit the power required through them.

Power is also transmitted by means of compressed air, an agent which, on the score of its ability to ventilate, and of its cleanliness, has much to recommend it. On the other hand, it is an agent which, having regard to the probability of the deposition of moisture in the form of ‘snow,’ requires to be worked with judgment.

Again, there is an alternative mode for the conveyance of power by the exhaustion of air—a mode which has been in practical use for over sixty years.

We have also the curious system pursued at Schaffhausen, where quick-running ropes are driven by turbines, these being worked by the current of the river Rhine; and at New York, and in other cities of the United States, steam is laid on under the streets, so as to enable domestic steam engines to be worked, without the necessity of a boiler, a stoker, or a chimney, the steam affording also means of heating the house when needed.

Lastly, there is the system of transmitting power by electricity, to which I have already adverted. I was glad to learn, only the other day, that there was every hope of this power being applied to the working of an important subterranean tramway.

These distributions from central sources need, as a rule, statutory powers to enable the pipes or wires to be placed under the roads; and, following the deplorable example of the Electrical Facilities Act, it is now the habit of the enlightened corporation and the enterprising town clerk of most boroughs to say to capitalists who are willing to embark their capital in the plant for the distribution of power from a central source—for their own profit, no doubt, but also, no doubt, for the good of the

community—‘We will oppose you in Parliament, unless you will consent that, at the end of twenty-one years, we may acquire compulsorily your property, and may do so, if it turns out to be remunerative, without other payment than that for the mere buildings and plant at that time existing.’ This is the way English enterprise is met, and then English engineers are taunted, by Englishmen—often by the very men who have had a share in making this ‘boa-constrictor’ of a ‘Facilities Act’—that their energy is not to be compared with that which is to be found in the United States and other countries. Again, however, I must remember that I am not addressing Section F.

There is one application of science, by engineers, which is of extreme beauty and interest, and that cannot be regarded with indifference by the agriculturists of this country. I allude to the Heat-withdrawing Engines (I should like to say, ‘Cold-Producers,’ but I presume, if I did, I should be criticised), which are now so very extensively used for the importation of fresh meat, and for its storage when received here. It need hardly be said, that that which will keep cool and sweet the carcasses of sheep will equally well preserve milk, and many other perishable articles of food. We have in these machines daily instances that, if you wish to make a ship’s hold cold, you can do it by burning a certain quantity of coals—a paradox, if ever there was one.

In this climate of ours, where the summer has been said to consist of ‘three hot days and a thunderstorm,’ there is hardly need to make a provision for cooling our houses, although there is an undoubted need for making a provision to heat them. Nevertheless, those of us who have hot-water heating arrangements for use in the winter would be very glad indeed if, without much trouble or expense, they could turn these about, so as to utilise them for cooling their houses in summer. Mr. Loftus Perkins, so well known for his labours in the use of very high-pressure steam (600 to 1,000 lbs. on the inch), and also so well known for those most useful high-pressure warming arrangements which, without disfiguring our houses by the passage of large pipes, keep them in a state of warmth and comfort throughout the winter, has lately taken up the mode of, I will say it, producing ‘cold’ by the evaporation of ammonia, and, by improvements in detail, has succeeded in making an apparatus which, without engine or pumps, produces ‘cold’ for some hours in succession, and requires, to put it in action, the preliminary combustion of only a few pounds of coke or a few feet of gas.

As I have said, our climate gives us but little need to provide or employ apparatus to cool our houses, but one can well imagine that the Anglo-Indian will be glad to give up his punkah for some more certain, and less draughty, mode of cooling.

I now desire to point out how, as the work of the engineer grows, his needs increase. New material, or better material of the old kind, has to be found to enable him to carry out these works of greater magni-

tude. At the beginning of this century, stone, brick, and timber were practically the only materials employed for that which I may call standing engineering work—*i.e.* buildings, bridges, aqueducts, and so on—while timber, cast iron, and wrought iron were for many years the only available materials for the framing and principal parts of moving machines and engines, with the occasional use of lead for the pipes and of copper for pipes and for boilers.

As regards the cast iron, little was known of the science involved (or that ought to be involved) in its manufacture. It was judged of by results. It was judged of largely by the eye. It was 'white,' it was 'mottled,' it was 'grey.' It was known to be 'fit for refining,' fit for 'strong castings,' or fit for castings in which great fluidity in the molten metal was judged to be of more importance than strength in the finished casting. With respect to wrought iron, it was judged of by its results also. It was judged of by the place of its manufacture—but when the works of the district were unknown, the iron, on being tested, was classed as 'good fibrous,' although some of the very best was 'steel-like,' or 'bad,' 'hot-short,' or 'cold-short.' A particular district would produce one kind of iron, another district another kind of iron. The ore, the flux, and the fuel were all known to have influence, but to what extent was but little realised; and if there came in a new ore, or a new flux, it might well be that for months the turn-out of the works into which these novelties had been introduced would be prejudiced. Steel again—that luxury of the days of my youth—was judged by the eye. The wrought bars, made into 'blister' steel by 'cementation,' were broken, examined, and grouped accordingly. Steel was known, no doubt, to be a compound of iron and carbon, but the importance of exactness in the percentage was but little understood, nor was it at all understood how the presence of comparatively small quantities of foreign matter might necessitate the variation of the proportions of carbon. The consequence was that anomalous results every now and then arose to confound the person who had used the steel, and falsifying the proverb 'true as steel,' steel became an object of distrust. Is it too much to say that Bessemer's great invention of steel made by the 'converter,' and that Siemens's invention of the open-hearth process, reacted on pure science, and set scientific men to investigate the laws which regulate the union of metals and of metalloids?—and that the labours of these scientific men have improved the manufacture, so that steel is now thoroughly and entirely trusted? By its aid engineering works are accomplished which, without that aid, would have been simply impossible. The Forth Bridge, the big gun, the compound armour of the ironclad with its steel face, the projectile to pierce that steel face—all equally depend upon the 'truth' of steel as much as does the barely visible hair spring of the chronometer, which enables the longitude of the ship in which it is carried to be ascertained. Now, what makes the difference between trustworthy and untrustworthy steel

for each particular purpose? Something which, until our better sense comes to our aid, we are inclined to look upon as ridiculously insignificant—a ‘next-to-nothing.’ Setting extraneous ingredients aside, and considering only the union of iron and carbon, the question whether there shall be added or deducted one-tenth of 1 per cent. (pardon my clumsy way of using the decimal system) of carbon is a matter of great importance in the resulting quality of the steel. This is a striking practical instance of how apparently insignificant things may be of the highest importance. The variation of this fraction of a percentage may render your boiler steel untrustworthy, may make the difference between safety in a gun and danger in a gun, and may render your armour-piercing projectile unable to pierce even the thinnest wrought-iron armour.

While thus brought incidentally to the subject of guns, let me derive from it another instance of the value of small things. I have in my hand a piece of steel ribbon. It is probable that only those who are near to me can see it. Its dimensions are one-fourth by one-sixteenth of an English inch, equal to an area of one sixty-fourth of a square inch. This mode of stating the dimensions I use for the information of the ladies. To make it intelligible to my scientific friends, I must tell them that it is approximately $\cdot 00637$ of a metre, by approximately $\cdot 00159$ of a metre, and that its sectional area is $\cdot 0000101283$ (also approximately) of a square metre. This insignificant (and speaking in reference to the greater number of my audience), practically invisible piece of material—that I can bend with my hand, and even tie into knots—is, nevertheless, not to be despised. By it one reinforces the massive and important-looking A-tube of a 9·2-inch gun, so that from that tube can be projected with safety a projectile weighing 380 pounds at a velocity, when leaving the muzzle, of between one-third and one-half of a mile in a second, and competent to traverse nearly $12\frac{1}{2}$ miles before it touches the ground. It may be said, ‘What is the use of being able to fire a projectile to a distance which commonly is invisible (from some obstacle or another) to the person directing the gun?’ I will suggest to you a use. Imagine a gun of this kind placed by some enemy who, unfortunately, had invaded us, and had reached Richmond. He has the range table for his gun; he, of course, is provided with our Ordnance maps, and he lays and elevates the gun at Richmond, with the object of striking, say, the Royal Exchange. Suppose he does not succeed in his exact aim. The projectile goes 100 yards to one side or to the other; or it falls 250 yards short, or passes 250 yards over; and it would be ‘bad shooting’ indeed, in these days, if nearly every projectile which was fired did not fall somewhere within an area such as this. In this suggested parallelogram of 100,000 square yards, or some 20 acres, there is some rather valuable property; and the transactions which are carried on are not unimportant. It seems to me that business would not be conducted with that calmness and coolness which are necessary for success, if, say every five minutes, a 380-pound

shell fell within this area, vomiting fire, and scattering its walls in hundreds of pieces, with terrific violence, in all directions. Do not suppose I am saying that similar effects cannot be obtained from a gun where wire is not employed. They can be. But my point is, that they can also be obtained by the aid of the insignificant thing which I am holding up at this moment—this piece of steel ribbon, which looks more suitable for the framework of an umbrella.

I have already spoken to you, when considering steel as a mere alloy of iron, as to the value of even a fraction of 1 per cent. of the carbon; but we know that in actual practice steel almost always contains other ingredients. One of the most prominent of these is manganese. It had for years been used, in quantities varying from a fraction of 1 per cent. up to 2·5 per cent., with advantage as regards ductility, and as regards its ability to withstand forging. A further increase was found not to augment the advantage: a still further increase was found to diminish it: and here the manufacturer stopped, and, so far as I know, the pure scientist stopped, on the very reasonable ground that the point of increased benefit appeared to have been well ascertained, and that there could be no advantage in pursuing an investigation which appeared only to result in decadence. But this is another instance of how the application of science reacts in the interests of pure science itself. One of our steel manufacturers, Mr. Hadfield, determined to pursue this apparently barren subject, and in doing so discovered this fact—that, while with the addition of manganese in excess of the limit before stated, and up to as much as 7 per cent., deterioration continued, after this latter percentage was passed improvement again set in.

Again, the effects of the addition of even the very smallest percentages of aluminium upon the steel with which it may be alloyed are very striking and very peculiar, giving to the steel alloy thus produced a very much greater hardness, and enabling it to take a much brighter and more silver-like polish. Further, the one-twentieth part of 1 per cent. of aluminium, when added to molten wrought iron, will reduce the fusing-point of the whole mass some 500 degrees, and will render it extremely fluid, and thus enable wrought iron (or what are commercially known as 'Mitis'—castings of the most intricate character) to be produced.

No one has worked more assiduously at the question of the effect of the presence of minute quantities, even traces, of alloys with metals than Professor Roberts-Austen, and he appears, by his experiments, to be discovering a general law, governing the effect produced by the mixture of particular metals, so that, in future, it is to be hoped, when an alloy is, for the first time, to be attempted, it will be possible to predict with reasonable certainty what the result will be, instead of that result remaining to be discovered by experiment.

I have just, incidentally, mentioned aluminium. May I say that we

engineers look forward, with much interest, to all processes tending to bring this metal, or its alloys, within possible commercial use?

One more instance of the effect of impurities in metals. The engineer engaged in electrical matters is compelled, in the course of his daily work, frequently to realise the importance of the 'next-to-nothing.' One striking instance of this is afforded by the influence which an extremely minute percentage of impurity has on the electrical conductivity of copper wire: this conductivity being in some cases reduced by as much as 50 per cent., in consequence of the admixture of that which, under other circumstances, would be looked upon as insignificant.

Reverting to the question of big guns. According to the present mode of manufacture, after we have rough-bored and turned the 'A' tube (and perhaps I ought to have mentioned that by the 'A' tube is meant the main piece of the gun, the innermost layer, if I may so call it, that portion which is the full length of the gun, and upon which the remainder of the gun is built up)—after, as I have said, we have rough-bored and turned this 'A' tube, we heat it to a temperature lying between certain specified limits, but actually determined by the behaviour of samples previously taken, and then suddenly immerse it perpendicularly into a well some 60 feet deep, full of oil, the oil in this well being kept in a state of change by the running into it, at the bottom, of cold oil conveyed by a pipe proceeding from an elevated oil tank. In this way the steel is oil-hardened, with the result of increasing its ultimate tensile strength, and also with the result of raising its so-called elastic limit. In performing this operation it is almost certain that injurious internal strains will be set up: strains tending to produce self-rupture of the material. Experiments have been carried out in England, by Captain Andrew Noble, and by General Maitland of the Royal Gun Factory, by General Kalakoutsky, in Russia, and also in the United States, to gauge what is the value, as represented by dimensions, of these strains, and we find that they have to be recorded in the most minute fractions of an inch, and yet, if the steel be of too 'high' a quality (as it is technically called), or if there has been any want of uniformity in the oil-hardening process, these strains, unless got rid of or ameliorated by annealing, may, as I have said, result in the self-rupture of the steel.

I have spoken of the getting rid of these strains by annealing, a process requiring to be conducted with great care, so as not to prejudice the effects of the oil-hardening. But take the case of a hardened steel projectile, hardened so that it will penetrate the steel face of compound armour. In that case annealing cannot be resorted to, for the extreme hardness of the projectile must not be in the least impaired. The internal strains in these projectiles are so very grave, that for months after they are made there is no security that they will not spontaneously fracture. I have here the point of an 8-inch projectile, which projectile weighs 210 lbs., this with others was received from the makers as long ago as March of

this year, and remained an apparently perfect and sound projectile until about the middle of August—some five months after delivery—and, of course, a somewhat longer time since manufacture—and between August 6th and 8th this piece which I hold in my hand, measuring $3\frac{3}{4}$ inches by $3\frac{1}{2}$ inches, spontaneously flew off from the rest of the projectile, and has done so upon a surface of separation which, whether having regard to its beautiful regularity, or to the conclusions to be drawn from it as to the nature of the strains existing, is of the very highest scientific interest. Many other cases of self-rupture of similar projectiles have been recorded.

Another instance of the effect of the ‘next-to-nothing’ in the hardening and tempering or annealing of steel. As we know, the iron and the carbon (leaving other matters out of consideration) are there. The carbon is (even in tool-steel) a very small proportion of the whole. The steel may be bent, and will retain the form given to it. You heat it and plunge it in cold water; you attempt to bend it and it breaks; but if, after the plunging in cold water, you temper it by carefully reheating it, you may bring it to the condition fit either for the cutting-tool for metal, or for the cutting-tool for wood, or for the watch-spring; and these important variations of condition which are thus obtained depend upon the ‘next-to-nothing’ in the temperature to which it is reheated, and therefore in the nature of the resulting combination of the ingredients of which the steel is composed.

Some admirable experiments were carried out on this subject by the Institution of Mechanical Engineers, with the assistance of one of our Vice-Presidents, Sir Frederick Abel, and the subject has also been dealt with by an eminent Russian writer.

There is, to my mind, another and very striking popular instance (if I may use the phrase) of the importance of attention to detail—that is, to the ‘next-to-nothing.’ Consider the bicycles and tricycles of the present day—machines which afford the means of healthful exercise to thousands, and which will, probably within a very short time, prove of the very greatest possible use for military purposes. The perfection to which these machines have been brought is almost entirely due to strict attention to detail; in the selection of the material of which the machines are made; in the application of pure science (in its strictest sense) to the form and to the proportioning of the parts, and also in the arrangement of these various parts in relation the one to the other. The result is that the greatest possible strength is afforded with only the least possible weight, and that friction in working has been reduced to a minimum. All of us who remember the hobby-horse of former years, and who contrast that machine with the bicycle or tricycle of the present day, realise how thoroughly satisfactory is the result of this attention to detail—this appreciation of the ‘next-to-nothing.’

Let me give you another illustration of the importance of small things, drawn from gunnery practice.

At first sight one would be tempted to say that the density of the air on the underside of a shot must, notwithstanding its motion of descent, be so nearly the same as that of the air upon the upper side as to be unworthy of consideration, but we know that the projectiles from rifled guns tend to travel sideways as they pass through the air, and that the direction of their motion, whether to the right or to the left, depends on the 'hand' of the rifling. We know also, that the friction against liquid or against gaseous bodies varies with the densities of these bodies, and it is believed that, minute as is the difference in density to which I have referred, it is sufficient to determine the lateral movement of the projectile. This lateral tendency must be allowed for, in these days of long ranges, in the sighting and laying of guns, if we desire accuracy of aim, at those distances at which it is to be expected our naval engagements will have to be commenced, and perhaps concluded. We can no longer afford to treat the subject as Nelson is said to have treated it, in one of his letters to the Secretary of the Admiralty, who had requested that an invention for laying guns more accurately should be tried. Nelson said he would be glad to try the invention, but that, as his mode of fighting consisted in placing his ship close alongside that of the enemy, he did not think the invention, even if it were successful, would be of much use to him.

While upon the question of guns, I am tempted to remark upon that which is by no means a small thing (for it is no less than the rotation of the earth), which in long-distance firing may demand attention, and that to an extent little suspected by the civilian.

Place the gun north and south, say in the latitude of London, and fire a 12-mile round such as I have mentioned, and it will be found that, assuming the shot were passing through a vacuum, a lateral allowance of more than 200 feet must be made to compensate for the different velocity of the circumference of the earth at 12 miles north or south of the place where the gun was fired, as compared with the velocity of the circumference of the earth at that place itself—the time of flight being in round numbers one minute.

At the risk of exciting a smile, I am about to assert that engineering has even its poetical side. I will ask you to consider with me whether there may not be true poetry in the feelings of the engineer who solves a problem such as this: Consider this rock, never visible above the surface of the tide, but making its presence known by the waves which rise around it: it has been the cause of destruction to many a noble vessel which had completed, in safety, its thousands of leagues of journey, and was, within a few score miles of port; then dashed to pieces upon it? Here is this rock. On it build a lighthouse. Lay your foundations through the water, in the midst of the turmoil of the sea; make your preparations; appear to be attaining success, and find the elements are against you and that the whole of your preliminary works are ruined

or destroyed in one night ; but again commence, and then go on and go on until at last you conquer ; your works rise above ordinary tide-level ; then upon these sure foundations, obtained it may be after years of toil amid the poetical surroundings I have indicated, erect a fair shaft, graceful as a palm and sturdy as an oak ; surmount it with a light, itself the produce of the highest application of science ; direct that light by the built-up lens, again involving the highest application of science ; apply mechanism, so arranged that the lighthouse shall from minute to minute reveal to the anxious mariner its exact name and its position on the coast. When you have done all this, will you not be entitled to say to yourself, 'It is I who have for ever rendered innocuous this rock which has been hitherto a dread source of peril' ? Is there no feeling, do you think, of a poetical nature excited in the breast of the engineer who has successfully grappled with a problem such as this ?

Another instance : the mouth of a broad river, or, more properly speaking, the inlet of the sea, has to be crossed at such a level as not to impede the passage of the largest ships. Except in one or two places the depth is profound, so that multiple foundations for supporting a bridge become commercially impossible, and the solution of the problem must be found by making, high in the air, a flight of span previously deemed unattainable. Is there no poetry here ? Again, although the results do not strike the eye in the same manner, is there nothing of poetry in the work, that has to be thought out and achieved, when a wide river or an ocean channel has to be crossed by a subterranean passage ? Works of great magnitude of this character have been performed with success, and to the benefit of those for whose use they were intended. One of the greatest and most noble of such works, encouraged, in years gone by, by the Governments of our own country and of France, has lately fallen into disfavour with an unreasoning public, who have not taken the pains to ascertain the true state of the case.

Surely it will be agreed that the promotion of ready intercourse and communication between nations constitute the very best, and most satisfactory guarantees for the preservation of peace ; when the peoples of two countries come to know each other intimately, and when they, therefore, enter into closer business relations, they are less liable to be led away by panic or by anger, and they hesitate to go to war the one with the other. It is in the interests of both that questions of difference which may arise between them should be amicably settled, and having an intimate knowledge of each other, they are less liable to misunderstand, and the mode of determination of their differences is more readily arranged. Remember, the means of ready intercourse and of communication, and the means of easy travel, are all due to the application of science by the engineer. Is not therefore his profession a beneficent one ?

Further, do you not think poetical feeling will be excited in the breast of that engineer who will in the near future solve the problem (and it

certainly will be solved when a sufficiently light motor is obtained) of travelling in the air—whether this solution be effected by enabling the self-suspended balloon to be propelled and directed, or perhaps, better still, by enabling not only the propulsion to be effected and the direction to be controlled, but by enabling the suspension in the air itself to be attained by mechanical means?

Take other functions of the Civil Engineer—functions which, after all, are of the most important character, for they contribute directly to the prevention of disease, and thereby not only prolong life, but do that which is probably more important—afford to the population a healthier life while lived.

In one town, about which I have full means of knowing, the report has just been made that in the year following the completion of a comprehensive system of sewerage, the deaths from zymotic diseases had fallen from a total of 740 per annum to a total of 372—practically one half. Has the engineer no inward satisfaction who knows such results as these have accrued from his work?

Again, consider the magnitude and completeness of the water supply of a large town, especially a town that has to depend upon the storing-up of rain water: the prevision which takes into account, not merely the variation of the different seasons of the year, but the variation of one year from another; that, having collated all the stored-up information, determines what must be the magnitude of the reservoirs to allow for at least three consecutive dry years, such as may happen; and that finds the sites where these huge reservoirs may be safely built.

All these—and many other illustrations which I could put before you if time allowed—appear to me to afford conclusive evidence that, whether it be in the erection of the lighthouse on the lonely rock at sea; whether it be in the crossing of rivers or seas, or arms of seas, by bridges or by tunnels; whether it be the cleansing of our towns from that which is foul; whether it be the supply of pure water to every dwelling, or the distribution of light or of motive power; or whether it be in the production of the mighty ocean steamer, or in the spanning of valleys, the piercing of mountains, and affording the firm, secure road for the express train; or whether it be the encircling of the world with telegraphs—the work of the Civil Engineer is not of the earth earthy, is not mechanical to the exclusion of science, is not unintellectual; but is of a most beneficent nature, is consistent with true poetical feeling, and is worthy of the highest order of intellect.

British Association for the Advancement of Science.

NEWCASTLE-UPON-TYNE, 1889.

ADDRESS

BY

PROFESSOR W. H. FLOWER,

C.B., LL.D., F.R.S., F.R.C.S., PRES.Z.S., F.L.S., F.G.S.,

PRESIDENT.

It is twenty-six years since this Association met in Newcastle-upon-Tyne. It had then the advantage of being presided over by one of the most distinguished and popular of your fellow-townsmen.

Considering the age usually attained by those upon whom the honour of the presidency falls, and the length of time which elapses before the Association repeats its visit, it must have rarely happened that any one who has held the office is spared, not only to be present at another meeting in the town in which he has presided, but also to take such an active part in securing its success, and to extend such a hospitable welcome to his successor, as Lord Armstrong has done upon the present occasion.

The address which was delivered at that meeting must have been full of interest to the great majority of those present. It treated of many subjects more or less familiar and important to the dwellers in this part of the world, and it treated them with the hand of a master, a combination which always secures the attention of an audience.

When it came to my knowledge that in the selection of the President for this meeting the choice had fallen upon me, I was filled with apprehension. There was nothing in my previous occupations or studies from which I felt that I could evolve anything in special sympathy with what is universally recognised as the prevailing genius of this district. I was, however, somewhat reassured when reminded that in the regular rotation by which the equal representation in the presidential office of the different branches of science included in the Association is secured, the turn had come round for some one connected with biological subjects to occupy the chair, which during the past seven years has been filled with such distinction by engineers, chemists, physicists, mathematicians, and geologists.

I was also reminded that the Association, though of necessity holding its meeting in some definite locality, was by no means local in its character, but that its sphere was co-extensive, not with the United Kingdom only, but with the whole of the British Dominions, and that our proceedings are followed with interest wherever our language is understood—I may say, throughout the civilised world. Furthermore, although its great manufacturing industries, the eminence of its citizens for their skill and intelligence in the practical application of mechanical sciences, and the interesting and important geological features of its vicinity, have conferred such fame on Newcastle as almost to have overshadowed its other claims to distinction in connection with science, this neighbourhood is also associated with Bewick, with Johnson, with Alder, Embleton, Hutton, Atthey, Norman, the two Hancocks, the two Bradys, and other names honoured in the annals of biology; it has long maintained a school of medicine of great repute; and there has lately been established here a natural history museum, which in some of its features is a model for institutions of the kind, and which, I trust, will be a means of encouraging in this town some of the objects the Association was designed to promote.

There can be no doubt that among the various methods by which the aims of the British Association (as expressed in its full title, the *advancement of science*) may be brought about, the collection and preservation of objects available for examination, study, and reference—in fact, the formation of what are now called ‘museums’—is one of very great practical importance; so much so, indeed, that it seems to me one to the consideration of which it is desirable to devote some time upon such an occasion as this. It is a subject still little understood, though, fortunately, beginning to attract attention. It has already been brought before the notice of the Association, both in presidential and sectional addresses. A committee of our members is at the present time engaged in collecting evidence upon it, and has issued some valuable reports. During the present year an association of curators and others interested in museums has been founded for the purpose of interchange of ideas upon the organisation and management of these institutions. It is a subject, moreover, if I may be allowed to mention a personal reason for bringing it forward this evening, which has more than any other occupied my time and my attention almost from the earliest period of my recollection, and I think you will agree with the opinion of one of my distinguished predecessors in this chair, ‘that the holder of this office will generally do better by giving utterance to what has already become part of his own thought than by gathering matter outside of its habitual range for the special occasion. For,’ continued Mr. Spottiswoode, ‘the interest (if any) of an address consists not so much in the multitude of things therein brought forward as in the individuality of the mode in which they are treated.’

The first recorded institution which bore the name of museum, or temple or haunt of the Muses, was that founded by Ptolemy Soter at Alexandria about 300 B.C.; but this was not a museum in our sense of the word, but rather, in accordance with its etymology, a place appropriated to the cultivation of learning, or which was frequented by a society or academy of learned men devoting themselves to philosophical studies and the improvement of knowledge.

Although certain great monarchs, as Solomon of Jerusalem and Augustus of Rome, displayed their taste and their magnificence by assembling together in their palaces curious objects brought from distant parts of the world—although it is said that the liberality of Philip and Alexander supplied Aristotle with abundant materials for his researches—of the existence of any permanent or public collections of natural objects among the ancients there is no record. Perhaps the nearest approach to such collections may be found in the preservation of remarkable specimens, sometimes associated with superstitious veneration, sometimes with strange legendary stories, in the buildings devoted to religious worship. The skins of the gorillas brought by the navigator Hanno from the West Coast of Africa, and hung up in the temple at Carthage, afford a well-known instance.

With the revival of learning in the Middle Ages, the collecting instinct, inborn in so many persons of various nations and periods of history, but so long in complete abeyance, sprang into existence with considerable vigour, and a museum, now meaning a collection of miscellaneous objects, antiquities as well as natural curiosities, often associated with a gallery of sculpture and painting, became a fashionable appendage to the establishment of many wealthy persons of superior culture.

All the earliest collections, comparable to what we call museums, were formed by and maintained at the expense of private individuals; sometimes physicians, whose studies naturally led them to a taste for biological science; often great merchant princes, whose trading connections afforded opportunities for bringing together things that were considered curious from foreign lands; or ruling monarchs in their private capacity. In every case they were maintained mainly for the gratification of the possessor or his personal friends, and rarely, if ever, associated with any systematic teaching or public benefit.

One of the earliest known printed catalogues of such a museum is that of Samuel Quicquelberg, a physician of Amsterdam, published in 1565 in Munich. In the same year Conrad Gesner published a catalogue of the collection of Johann Kentmann, a physician of Torgau in Saxony, consisting of about 1,600 objects, chiefly minerals, shells, and marine animals. Very soon afterwards we find the Emperor Rudolph II. of Germany busily accumulating treasures which constituted the foundations of the present magnificent museums by which the Austrian capital is distinguished.

In England the earliest important collectors of miscellaneous objects

were the two John Tradescants, father and son, the latter of whom published, in 1656, a little work called 'Musæum Tradescantianum; or, a Collection of Rarities preserved at South Lambeth near London.' The wonderful variety and incongruous juxtaposition of the objects contained in this collection make the catalogue very amusing reading. Under the first division, devoted to 'Some Kindes of Birds, their Egges, Beaks, Feathers, Clawes and Spurres,' we find 'Divers sorts of Egges from Turkie, one given for a Dragon's Egge'; 'Easter Egges of the Patriarch of Jerusalem'; 'Two Feathers of the Phoenix Tayle'; 'The Claw of the bird Rock, who, as Authors report, is able to trusse an Elephant.' Among 'whole birds' is the famous 'Dodar from the Island Mauritius; it is not able to flie, being so big.' This is the identical specimen, the head and foot of which has passed through the Ashmolean into the University Museum of Oxford; but we know not what has become of the claw of the Rock, the Phoenix tayle, and the Dragon's egg. Time does not allow me to mention the wonderful things which occur under the head of 'Garments, Vestures, Habits, and Ornaments,' or the 'Mechanick, Artificial Workes in Carvings, Turnings, Sowings, and Paintings,' from Edward the Confessor's knit gloves, and the famous 'Pohatan, King of Virginia's habit, all embroidered with shells or Roanoke,' also still at Oxford, and lately figured and described by Mr. E. B. Tylor, to the 'Cherry-stone, upon one side S. George and the Dragon, perfectly cut, and on the other side 88 Emperours' faces'; or the other 'cherry-stone, holding ten dozen of tortois-shell combs made by Edward Gibbons.' But before leaving these private collections I cannot forbear mentioning, as an example of the great aid they often were in advancing science, the indebtedness of Linnæus in his early studies to the valuable zoological museums, which it was one of the ruling passions of several kings and queens of Sweden to bring together.

Upon the association of individuals together into societies to promote the advancement of knowledge, these bodies in their corporate capacity frequently made the formation of a museum part of their function. The earliest instance of this in our country was the museum of the Royal Society in Crane Court, of which an illustrated catalogue was published by Dr. Grew in 1681.

The idea that the maintenance of a museum was a portion of the public duty of the State or of any municipal institution had, however, nowhere entered into the mind of man at the beginning of the last century. Even the great teaching bodies, the Universities, were slow in acquiring collections; but it must be recollected that the subjects considered most essential to the education they then professed to give were not those which needed illustration from the objects which can be brought together in a museum. The Italian Universities, where anatomy was taught as a science earlier and more thoroughly than anywhere else in Europe, soon found the desirability of keeping collections of preserved

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specimens, and the art of preparing them attained a high degree of excellence at Padua and Bologna two centuries ago. But these were generally the private property of the professors, as were nearly all the collections used to illustrate the teaching of anatomy and pathology in our country within the memory of many now living.

Notwithstanding the multiplication of public museums during the present century, and the greater resources and advantages which many of these possess, which private collectors cannot command, the spirit of accumulation in individuals has happily not passed away, although usually directed into rather different channels than formerly. The general museums or miscellaneous collections of old are now left to governments and institutions which afford greater guarantee of their permanence and public utility, while admirable service is done to science by those private persons with leisure and means who, devoting themselves to some special subject, amass the materials by which its study can be pursued in detail either by themselves or by those they know to be qualified to do so; which collections, if they fulfil their most appropriate destiny, ultimately become incorporated, by gift or purchase, in one or other of the public museums, and then serve as permanent factors in the education of the nation, or rather of the world.

It would be passing beyond the limits of time allotted to this address, indeed going beyond the scope of the Association, if I were to speak of many of the subjects which have pre-eminently exercised the faculties of the collector and formed the materials of which museums are constructed. The various methods by which the mind of man has been able to reproduce the forms of natural objects or to give expression to the images created by his own fancy, from the rudest scratchings of a savage on a bone, or the simplest arrangement of lines employed in ornamenting the roughest piece of pottery, up to the most lovely combinations of form and colour hitherto attained in sculpture or in painting, or in works in metal or in clay, depend altogether on museums for their preservation, for our knowledge of their condition and history in the past, and for the lessons which they can convey for the future.

Apart from the delight which the contemplation of the noblest expressions of art must produce in all cultivated minds, apart also from the curiosity and interest that must be excited by all the less successfully executed attempts to produce similar results, as materials for constructing the true history of the life of man, at different stages of civilisation, in different circumstances of living, and in divers regions of the earth, such collections are absolutely invaluable.

But I must pass them by in order to dwell more in detail upon those which specially concern the advancement of the subjects which come under the notice of this Association—museums devoted to the so-called ‘natural history’ sciences, although much will be said of them which will doubtless be more or less applicable to museums in general.

The terms '*natural history*' and '*naturalist*' have become deeply rooted in our language, but without any very definite conception of their meaning or the scope of their application. Originally applied to the study of all the phenomena of the universe which are independent of the agency of man, natural history has been broken down in most people's minds, in consequence of the invention of convenient and generally understood and accepted terms for some of its various subdivisions, as anatomy, chemistry, geology, &c., into that portion of the subject which treats of the history of creatures endowed with life, for which, until lately, no special name had been invented. Even from this limitation botany was gradually disassociating itself in many quarters, and a '*naturalist*' and a '*zoologist*' have nearly become, however irrationally, synonymous terms. The happy introduction and general acceptance of the word '*biology*,' notwithstanding the objections raised to its etymological signification, have reunited the study of organisms distinguished by the possession of the living principle, and practically eliminated the now vague and indefinite term '*natural history*' from scientific terminology. As, however, it is certain to maintain its hold in popular language, I would venture to suggest the desirability of restoring it to its original and really definite signification, contrasting it with the history of man and of his works, and of the changes which have been wrought in the universe by his intervention.

It was in this sense that, when the rapid growth of the miscellaneous collections in the British Museum at Bloomsbury (the expansion of Sir Hans Sloane's accumulation in the old Manor House at Chelsea) was thought to render a division necessary, the line of severance was effected at the junction of what was natural and what was artificial; the former, including the products of what are commonly called '*natural*' forces, unaffected by man's handiwork, or the impress of his mind. The departments which took cognisance of these were termed the '*Natural History Departments*,' and the new building to which they were removed the '*Natural History Museum*.'

It may be worth while to spend a few moments upon the consideration of the value of this division, as it is one which concerns the arrangement and administration of the majority of museums.

Though there is very much to be said for it, the objection has been raised that it cuts man himself in two. The illustrations of man's bodily structure are undoubtedly subjects for the zoologist. The subtle gradations of form, proportion, and colour which distinguish the different races of men, can only be appreciated by one with the education of an anatomist, and whose eye has been trained to estimate the value of such characters in discriminating the variations of animal forms. The subjects for comparison required for this branch of research must therefore be looked for in the zoological collections.

But the comparatively new science of '*anthropology*' embraces not

only man's physical structure: it includes his mental development, his manners, customs, traditions, and languages. The illustrations of his works of art, domestic utensils, and weapons of war are essential parts of its study. In fact it is impossible to say where it ends. It includes all that man is or ever has been, all that he has ever done. No definite line can be drawn between the rudest flint weapon and the most exquisitely finished instrument of destruction which has ever been turned out from the manufactory at Elswick, between the rough representation of a mammoth, carved by one of its contemporary men on a portion of its own tusk, and the most admirable production of a Landseer. An anthropological collection, to be logical, must include all that is in not only the old British Museum but the South Kensington Museum and the National Gallery. The notion of an anthropology which considers savages and pre-historic people as apart from the rest of mankind may, in the limitations of human powers, have certain conveniences, but it is utterly unscientific and loses sight of the great value of the study in tracing the gradual growth of our complex systems and customs from the primitive ways of our progenitors.

On the other hand, the division first indicated is as perfectly definite, logical, and scientific as any such division can be. That there are many inconveniences attending wide local disjunctions of the collections containing subjects so distinct yet so nearly allied as physical and psychical anthropology must be fully admitted; but these could only have been overcome by embracing in one grand institution the various national collections illustrating the different branches of science and art, placed in such order and juxtaposition that their mutual relations might be apparent, and the resources of each might be brought to bear upon the elucidation of all the others—an ideal institution, such as the world has not yet seen, but into which the old British Museum might at one time have been developed.

A purely 'Natural History Museum' will then embrace a collection of objects illustrating the natural productions of the earth, and in its widest and truest sense should include, as far as they can be illustrated by museum specimens, all the sciences which deal with natural phenomena. It has only been the difficulties, real or imaginary, in illustrating them which have excluded such subjects as astronomy, physics, chemistry, and physiology from occupying departments in our National Natural History Museum, while allowing the introduction of their sister sciences, mineralogy, geology, botany, and zoology.

Though the experimental sciences and those which deal with the laws which govern the universe, rather than with the materials of which it is composed, have not hitherto greatly called forth the collector's instinct, or depended upon museums for their illustration, yet the great advantages of collections of the various instruments by means of which these sciences are pursued, and of examples of the methods by which they are taught, are

yearly becoming more manifest. Museums of scientific apparatus now form portions of every well-equipped educational establishment, and under the auspices of the Science and Art Department at South Kensington a national collection illustrating those branches of natural history science which have escaped recognition in the British Museum is assuming a magnitude and importance which brings the question of properly housing and displaying it urgently to the front.

Anomalies such as these are certain to occur in the present almost infantile though rapidly progressive state of science. It may be taken for granted that no scientific institution of any complexity of organisation can be, except at the moment of its birth, abreast of the most modern views of the subject, especially in the dividing lines between, and the proportional representation of, the various branches of knowledge which it includes.

The necessity for subdivisions in the study of science is continually becoming more apparent as the knowledge of the details of each subject multiplies without corresponding increase in the power of the human mind to grasp and deal with them, and the dividing lines not only become sharper, but as knowledge advances they frequently require revision. It might be supposed that such revision would adjust itself to the direction taken by the natural development of the relations of the different branches of science, and the truer conceptions entertained of such relations. But this is not always so. Artificial barriers are continually being raised to keep these dividing lines in the direction in which they have once started. Difficulties of readjustment arise not only from the mechanical obstacles caused by the size and arrangements of the buildings and facilities for the allocation of various kinds of collections, but still more from the numerous personal interests which grow up and wind their meshes around such institutions. Professorships and curatorships of this or that division of science are founded and endowed, and their holders are usually tenacious either of encroachment upon or of any wide enlargement of the boundaries of the subject they have undertaken to teach or to illustrate; and in this way, more than any other, passing phases of scientific knowledge have become crystallised or fossilised in institutions where they might least have been expected. I may instance many European universities and great museums in which zoology and comparative anatomy are still held to be distinct subjects taught by different professors, and where, in consequence of the division of the collections under their charge, the skin of an animal, illustrating its zoology, and its skeleton and teeth, illustrating its anatomy, must be looked for in different and perhaps remotely placed buildings.

For the perpetuation of the unfortunate separation of palæontology from biology, which is so clearly a survival of an ancient condition of scientific culture, and for the maintenance in its integrity of the heterogeneous compound of sciences which we now call 'geology,'

the faulty organisation of our museums is in a great measure responsible. The more their rearrangement can be made to overstep and break down the abrupt line of demarcation which is still almost universally drawn between beings which live now and those which have lived in past times, so deeply rooted in the popular mind and so hard to eradicate even in that of the scientific student, the better it will be for the progress of sound biological knowledge.

But it is not of the removal of such great anomalies and inconsistencies which, when they have once grown up, require heroic methods to set them right, but rather of certain minor defects in the organisation of almost all existing museums which are well within the capacity of comparatively modest administrative means to remedy, that I have now to speak.

That great improvements have been lately effected in many respects in some of the museums in this country, on the Continent, and especially in America, no one can deny. The subject, as I have already indicated, is, happily, exciting the attention of those who have the direction of them, and even awakening interest in the mind of the general public. It is in the hope of in some measure helping on or guiding this movement that I have ventured on the remarks which follow.

The first consideration in establishing a museum, large or small, either in a town, institution, society, or school, is that it should have some definite object or purpose to fulfil; and the next is that means should be forthcoming not only to establish but also to maintain the museum in a suitable manner to fulfil that purpose. Some persons are enthusiastic enough to think that a museum is in itself so good an object that they have only to provide a building and cases and a certain number of specimens, no matter exactly what, to fill them and then the thing is done; whereas the truth is the work has only then begun. What a museum really depends upon for its success and usefulness is not its building, not its cases, not even its specimens, but its curator. He and his staff are the life and soul of the institution, upon whom its whole value depends; and yet in many—I may say most of our museums—they are the last to be thought of. The care, the preservation, the naming of the specimens are either left to voluntary effort—excellent often for special collections and for a limited time, but never to be depended on as a permanent arrangement—or a grievously undersalaried and consequently uneducated official is expected to keep in order, to clean, dust, arrange, name, and display in a manner which will contribute to the advancement of scientific knowledge, collections ranging in extent over almost every branch of human learning, from the contents of an ancient British barrow to the last discovered bird of paradise from New Guinea.

Valuable specimens not unfrequently find their way into museums thus managed. Their public-spirited owners fondly imagine that they will be preserved and made of use to the world if once given to such an

institution. Their fate is, unfortunately, far otherwise. Dirty, neglected, without label, their identity lost, they are often finally devoured by insects or cleared away to make room on the crowded shelves for the new donation of some fresh patron of the institution. It would be far better that such museums should never be founded. They are traps into which precious—sometimes priceless—objects fall only to be destroyed; and, what is still worse, they bring discredit on all similar institutions, make the very name of museum a byword and a reproach, hindering instead of advancing the recognition of their value as agents in the great educational movement of the age.

A museum is like a living organism—it requires continual and tender care. It must grow, or it will perish; and the cost and labour required to maintain it in a state of vitality is not yet by any means fully realised or provided for, either in our great national establishments or in our smaller local institutions.

Often as it has been said, it cannot be too often repeated, that the real objects of forming collections, of whatever kind (apart, of course, from the mere pleasure of acquisition—sometimes the only motive of private collectors), and which, although in very different degrees, and often without being recognised, underlie the organisation of all museums, are two, which are quite distinct, and sometimes even conflicting. The first is to advance or increase the knowledge of some given subject. This is generally the motive of the individual collector, whose experience shows him the vast assistance in forming definite ideas in any line of research in which he may be occupied that may be derived from having the materials for its study at his own command, to hold and to handle, to examine and compare, to take up and lay aside whenever the favourable moment to do so occurs. But unless his subject is a very limited one, or his means the reverse, he soon finds the necessity of consulting collections based on a larger scale than his own. Very few people have any idea of the multiplicity of specimens required for the purpose of working out many of the simplest problems concerning the life-history of animals or plants. The naturalist has frequently to ransack all the museums, both public and private, of Europe and America in the endeavour to compose a monograph of a single common genus, or even species, that shall include all questions of its variation, changes in different seasons, and under different climates and conditions of existence, and the distribution in space and time of all its modifications. He often has to confess at the end that he has been baffled in his research for want of the requisite materials for such an undertaking. Of course this ought not to be, and the time will come when it will not be, but that time is very far off yet.

We all know the old saying that the craving for riches grows as the wealth itself increases. Something similar is true of scientific collections brought together for the purpose of advancing knowledge. The larger they are the more their deficiencies seem to become conspicuous; the

more desirous we are to fill up the gaps which provokingly interfere with our extracting from them the complete story they have to tell.

Such collections are, however, only for the advanced student, the man who has already become acquainted with the elements of his science and is in a position, by his knowledge, by his training, and by his observing and reasoning capacity, to take advantage of such material to carry on the subject to a point beyond that at which he takes it up.

But there is another and a far larger class to whom museums are or should be a powerful means of aid in acquiring knowledge. Among such those who are commencing more serious studies may be included; but I especially refer to the much more numerous class, and one which it may be hoped will year by year bear a greater relative proportion to the general population of the country, who, without having the time, the opportunities, or the abilities to make a profound study of any branch of science, yet take a general interest in its progress, and wish to possess some knowledge of the world around them and of the principal facts ascertained with regard to it, or at least some portions of it. For such persons museums may be, when well organised and arranged, of benefit to a degree that at present can scarcely be realised.

To diffuse knowledge among persons of this class is the second of the two purposes of museums of which I have spoken.

—I believe that the main cause of what may be fairly termed the failure of the majority of museums—especially museums of natural history—to perform the functions that might be legitimately expected of them is that they nearly always confound together the two distinct objects which they may fulfil, and by attempting to combine both in the same exhibition practically accomplish neither.

In accordance with which of those two objects, which may be briefly called *research* and *instruction*, is the main end of the museum, so should the whole be primarily arranged; and in accordance with the object for which each specimen is required, so should it be treated.

The specimens kept for research, for advancement of knowledge, for careful investigations in structure and development, or for showing the minute distinctions which must be studied in working out the problems connected with variations of species according to age, sex, season, or locality; for fixing the limits of geographical distribution, or determining the range in geological time, must be not only exceedingly numerous (so numerous, indeed, that it is almost impossible to put a limit on what may be required for such purposes), but they must also be kept under such conditions as to admit of ready and close examination and comparison.

If the whole of the specimens really required for enlarging the boundaries of zoological or botanical science were to be displayed in such a manner that each one could be distinctly seen by any visitor sauntering through the public galleries of a museum, the vastness and expense of the

institution would be out of all proportion to its utility; the specimens themselves would be quite inaccessible to the examination of all those capable of deriving instruction from them, and, owing to the injurious effects of continued exposure to light upon the greater number of preserved natural objects, would ultimately lose a large part of their permanent value. Collections of this kind must, in fact, be treated as the books in a library, and be used only for consultation and reference by those who are able to read and appreciate their contents. To demand, as has been ignorantly done, that all the specimens belonging to our national museums, for instance, should be displayed in cases in the public galleries, would be equivalent to asking that every book in a library, instead of being shut up and arranged on shelves for consultation when required, should have every single page framed and glazed and hung on the walls, so that the humblest visitor as he passes along the galleries has only to open his eyes and revel in the wealth of literature of all ages and all countries, without so much as applying to a custodian to open a case. Such an arrangement is perfectly conceivable. The idea from some points of view is magnificent, almost sublime. But imagine the space required for such an arrangement of the national library of books, or, indeed, of any of the smallest local libraries; imagine the inconvenience to the real student, the disadvantages which he would be under in reading the pages of any work fixed in an immovable position beneath a glass case; think of the enormous distances he would often have to traverse to compare a reference or verify a quotation, and the idea of sublimity soon gives place to its usual antithesis. The attempt to display every bird, every insect, shell, or plant which is or ought to be in any of our great museums of reference would produce an exactly similar result.

In the arrangement of collections designed for research, which, of course, will contain all those precious specimens called 'types,' which must be appealed to through all time to determine the species to which a name was originally given, the principal points to be aimed at are—the preservation of the objects from all influences deleterious to them, especially dust, light, and damp; their absolutely correct identification, and record of every circumstance that need be known of their history; their classification and storage in such a manner that each one can be found without difficulty or loss of time; and, both on account of expense as well as convenience of access, they should be made to occupy as small a space as is compatible with these requirements. They should be kept in rooms provided with suitable tables and good light for their examination, and within reach of the necessary books of reference on the particular subjects which the specimens illustrate. Furthermore, the rooms should be so situated that the officers of the museum, without too great hindrance to their own work, can be at hand for occasional assistance and supervision of the student, and if collections of research and exhibited specimens are contained in one building, it is obvious that the closer the contiguity

in which those of any particular group are placed the greater will be the convenience both of students and curators, for in very few establishments will it be possible to form each series on such a scale as to be entirely independent of the other.

On the other hand, in a collection arranged for the instruction of the general visitor, the conditions under which the specimens are kept should be totally different. In the first place, their numbers must be strictly limited, according to the nature of the subject to be illustrated and the space available. None must be placed too high or too low for ready examination. There must be no crowding of specimens one behind the other, every one being perfectly and distinctly seen, and with a clear space around it. Imagine a picture-gallery with half the pictures on the walls partially or entirely concealed by others hung in front of them; the idea seems preposterous, and yet this is the approved arrangement of specimens in most public museums. If an object is worth putting into a gallery at all it is worth such a position as will enable it to be seen. Every specimen exhibited should be good of its kind, and all available skill and care should be spent upon its preservation and rendering it capable of teaching the lesson it is intended to convey. And here I cannot refrain from saying a word upon the sadly neglected art of taxidermy, which continues to fill the cases of most of our museums with wretched and repulsive caricatures of mammals and birds, out of all natural proportions, shrunk here and bloated there, and in attitudes absolutely impossible for the creature to have assumed while alive. Happily there may be seen occasionally, especially where amateurs of artistic taste and good knowledge of natural history have devoted themselves to the subject, examples enough—and you are fortunate in possessing them in Newcastle—to show that an animal can be converted after death, by a proper application of taxidermy, into a real life-like representation of the original, perfect in form, proportions, and attitude, and almost, if not quite, as valuable for conveying information on these points as the living creature itself. The fact is that taxidermy is an art resembling that of the painter or rather the sculptor; it requires natural genius as well as great cultivation, and it can never be permanently improved until we have abandoned the present conventional low standard and low payment for ‘bird-stuffing,’ which is utterly inadequate to induce any man of capacity to devote himself to it as a profession.

To return from this digression, every specimen exhibited should have its definite purpose, and no absolute duplicate should on any account be permitted. Above all, the purpose for which each specimen is exhibited, and the main lesson to be derived from it, must be distinctly indicated by the labels affixed, both as headings of the various divisions of the series, and to the individual specimens. A well-arranged educational museum has been defined as a collection of instructive labels illustrated by well-selected specimens.

What is, or should be, the order of events in arranging a portion of a public museum? Not, certainly, as too often happens now, bringing a number of specimens together almost by haphazard, and cramming them as closely as possible in a case far too small to hold them, and with little reference to their order or to the possibility of their being distinctly seen. First, as I said before, you must have your curator. He must carefully consider the object of the museum, the class and capacities of the persons for whose instruction it is founded, and the space available to carry out this object. He will then divide the subject to be illustrated into groups, and consider their relative proportions, according to which he will plan out the space. Large labels will next be prepared for the principal headings, as the chapters of a book, and smaller ones for the various subdivisions. Certain propositions to be illustrated, either in the structure, classification, geographical distribution, geological position, habits, or evolution of the subjects dealt with, will be laid down and reduced to definite and concise language. Lastly will come the illustrative specimens, each of which as procured and prepared will fall into its appropriate place. As it is not always easy to obtain these at the time that they are wanted, gaps will often have to be left, but these, if properly utilised by drawings or labels, may be made nearly as useful as if occupied by the actual specimens.

A public exhibition which is intended to be instructive and interesting must never be crowded. There is, indeed, no reason why it ever should be. Every such exhibition, whether on a large or small scale, can only contain a representative series of specimens, selected with a view to the needs of the particular class of persons who are likely to visit the gallery, and the number of specimens exhibited should be adapted to the space available. There is, therefore, rarely any excuse for filling it up in such a manner as to interfere with the full view of every specimen shown. A crowded gallery, except in some very exceptional circumstances, at once condemns the curator, as the remedy is generally in his own hands. In order to avoid it he has nothing to do but sternly to eliminate all the less important specimens. If any of these possess features of historical or scientific interest demanding their permanent preservation, they should be kept in the reserve collections; if otherwise, they should not be kept at all.

The ideal public museums of the future will, however, require far more exhibition space than has hitherto been allowed; for though the number of specimens shown may be fewer than is often thought necessary now, each will require more room if the conditions above described are carried out, and especially if it is thought desirable to show it in such a manner as to enable the visitor to realise something of the wonderful complexity of the adaptations which bring each species into harmonious relation with its surrounding conditions. Artistic reproductions of natural environments, illustrations of protective resemblances, or of special modes

of life, all require much room for their display. This method of exhibition, wherever faithfully carried out, is, however, proving both instructive and attractive, and will doubtless be greatly extended.

Guide-books and catalogues are useful adjuncts, as being adapted to convey fuller information than labels, and as they can be taken away for study during the intervals of visits to the museum, but they can never supersede the use of labels. Anyone who is in the habit of visiting picture-galleries where the names of the artists and the subject are affixed to the frame, and others in which the information has in each case to be sought by reference to a catalogue, must appreciate the vast superiority in comfort and time-saving of the former plan.

Acting upon such principles as these, every public gallery of a museum, whether the splendid saloon of a national institution or the humble room containing the local collection of a village club, can be made a centre of instruction, and will offer interests and attractions which will be looked for in vain in the majority of such institutions at the present time.

One of the best illustrations of the different treatment of collections intended for research or advancement of knowledge, and for popular instruction or diffusion of knowledge, is now to be seen in Kew Gardens, where the admirably constructed and arranged herbarium answers the first purpose, and the public museums of economic botany the second. A similar distinction is carried out in the collections of systematic botany in the natural history branch of the British Museum, with the additional advantage of close contiguity; indeed, as an example of a scheme of good museum arrangement (although not perfect yet in details) I cannot do better than refer to the upper story of the east wing of that institution. The same principles, little regarded in former times in this country, and still unknown in some of the largest Continental museums, are gradually pervading every department of the institution, which, from its national character, its metropolitan position, and exceptional resources, ought to illustrate in perfection the ideal of a natural history museum. In fact, it is only in a national institution that an exhaustive research collection in all branches of natural history, in which the specialist of every group can find his own subject fully illustrated, can or ought to be attempted.

As the actual comparison of specimen with specimen is the basis of zoological and botanical research, and as work done with imperfect materials is necessarily imperfect in itself, it is far the wisest policy to concentrate in a few great central institutions, the number and situation of which must be determined by the population and the resources of the country, all the collections, especially those containing specimens already alluded to as so dear to the systematic naturalist, known as author's 'types,' required for original investigations. It is far more advantageous to the investigator to go to such a collection and take up his temporary

abode there, while his research is being carried out, with all the material required at his hand at once, than to travel from place to place and pick up piecemeal the information he requires, without opportunity of direct comparison of specimens.

I do not say that collections for special study, and even original research, should not, under particular circumstances and limitations, be formed at museums other than central national institutions, or that nothing should be retained in provincial museums but what is of a directly educational or elementary nature. A local collection, illustrating the fauna and flora of the district, should be part of every such museum; and this may be carried to almost any amount of detail, and therefore in many cases it would be very unadvisable to exhibit the whole of it. A selection of the most important objects may be shown under the conditions described above, and the remainder carefully preserved in cabinets for the study of specialists.

It is also very desirable in all museums, in order that the exhibited series should be as little disturbed as possible in arrangement, and be always available for the purpose for which it is intended, that there should be, for the use of teachers and students, a supplementary set of common objects, which, if injured, could be easily replaced. It must not be forgotten that the zealous investigator and the conscientious curator are often the direst antagonists: the one endeavours to get all the knowledge he can out of a specimen, regardless of its ultimate fate, and even if his own eyes alone have the advantage of it; the other is content if a limited portion only is seen, provided that can be seen by everyone both now and hereafter.

Such, then, is the primary principle which ought to underlie the arrangement of all museums—the distinct separation of the two objects for which collections are made; the publicly exhibited collection being never a store-room or magazine, but only such as the ordinary visitor can understand and profit by, and the collection for students being so arranged as to afford every facility for examination and research. The improvements that can be made in detail in both departments are endless, and to enter further into their consideration would lead me far beyond the limits of this address. Happily, as I said before, the subject is receiving much attention.

I would willingly dwell longer upon it—indeed I feel that I have only been able to touch slightly and superficially upon many questions of practical interest, well worthy of more detailed consideration—but time warns me that I must be bringing this discourse to a close, and I have still said nothing in reference to subjects upon which you may expect some words on this occasion. I mean those great problems concerning the laws which regulate the evolution of organic beings, problems which agitate the minds of all biologists of the present day, and the solution of which is watched with keen interest by a far wider circle—a circle, in

fact, coincident with the intelligence and education of the world. Several communications connected with these problems will be brought before the sectional meetings during the next few days, and we shall have the advantage of hearing them discussed by some of those who by virtue of their special attention to and full knowledge of these subjects are most competent to speak with authority. It is therefore for me rather delicate ground to tread upon, especially at the close of a discourse mainly devoted to another question. I will, however, briefly point out the nature of the problems and the lines which the endeavour to solve them will probably take, without attempting to anticipate the details which you will doubtless hear most fully and ably stated elsewhere.

I think I may safely premise that few, if any, original workers at any branch of biology appear now to entertain serious doubt about the general truth of the doctrine that all existing forms of life have been derived from other forms by a natural process of descent with modification, and it is generally acknowledged that to the records of the past history of life upon the earth we must look for the actual confirmation of the truth of a doctrine which accords so strongly with all we know of the present history of living beings.

Professor Huxley wrote in 1875: 'The only perfectly safe foundation for the doctrine of evolution lies in the historical, or rather archæological, evidence that particular organisms have arisen by the gradual modification of their predecessors, which is furnished by fossil remains. That evidence is daily increasing in amount and in weight, and it is to be hoped that the comparisons of the actual pedigree of these organisms with the phenomena of their development may furnish some criterion by which the validity of phylogenic conclusions deduced from the facts of embryology alone may be satisfactorily tested.'

Palæontology, however, as we all know, reveals her secrets with no open hand. How can we be reminded of this more forcibly than by the discovery announced scarcely three months ago by Professor Marsh of numerous mammalian remains from formations of the Cretaceous period, the absence of which had so long been a source of difficulty to all zoologists? What vistas does this discovery open of future possibilities, and what thorough discredit, if any were needed, does it throw on the value of negative evidence in such matters! Bearing fully in mind the necessary imperfection of the record we have to deal with, I think that no one taking an impartial survey of the recent progress of palæontological discovery can doubt that the evidence in favour of a gradual modification of living forms is still steadily increasing. Any regular progressive series of changes of structure coinciding with changes in time can of course only be expected to be preserved and to come again before our eyes under such a favourable combination of circumstances as must be of most rare occurrence; but the links, more or less perfect, of many such series are continually being revealed, and the discovery of a single inter-

mediate form is often of immense interest as indicating the path along which the modification from one apparently distinct form to another may have taken place.

Though palæontology may be appealed to in support of the conclusion that modifications have taken place as time advanced, it can scarcely afford any help in solving the more difficult problems which still remain as to the methods by which the changes have been brought about.

Ever since the publication of what has been truly described as the 'creation of modern natural history,' Darwin's work on the 'Origin of Species,' there has been no little controversy as to how far all the modifications of living forms can be accounted for by the principle of natural selection or preservation of variations best adapted for their surrounding conditions, or whether any, and if so what, other factors have taken part in the process of organic evolution.

It certainly cannot be said that in these later times the controversy has ended. Indeed those who are acquainted with scientific literature must know that notes struck at the last annual meeting of this Association produced a series of reverberations, the echoes of which have hardly yet died away.

Within the last few months also two important works have appeared in our country, which have placed in an accessible and popular form many of the data upon which the most prevalent views on the subject are based.

The first is 'Darwinism: an Exposition of the Theory of Natural Selection, with some of its Applications,' by Alfred Russel Wallace. No one could be found so competent to give such an exposition of the theory as one who was, simultaneously with Darwin, its independent originator, but who, by the title he has chosen no less than by the contents of the book, has, with rare modesty and self-abnegation, transferred to his fellow-labourer all the merit of the discovery of what he evidently looks upon as a principle of overwhelming importance in the economy of nature; 'supreme,' indeed, he says, 'to an extent which even Darwin himself hesitated to claim for it.'

The other work I refer to is the English translation of the remarkable 'Essays upon Heredity and Kindred Biological Problems,' by Dr. August Weismann, published at the Oxford Clarendon Press, in which is fully discussed the very important but still open question—a question which was brought into prominence at our meeting at Manchester two years ago—of the transmission or non-transmission to the offspring of characters acquired during the lifetime of the parent.

It is generally recognised that it is one of the main elements of Darwin's, as well as of every other theory of evolution, that there is in every individual organic being an innate tendency to vary from the standard of its predecessors, but that this tendency is usually kept under the sternest control by the opposite tendency to resemble them, a force to which the terms 'heredity' and 'atavism' are applied. The causes

of this initial tendency to vary, as well as those of its limits and prevailing direction, and the circumstances which favour its occasional bursting through the constraining principle of heredity offer an endless field for speculation. Though several theories of variation have been suggested, I think that no one would venture to say we have passed beyond the threshold of knowledge of the subject at present.

Taking for granted, however, as we all do, that this tendency to individual variation exists, then comes the question, What are the agents by which, when it has asserted itself, it is controlled or directed in such a manner as to produce the permanent or apparently permanent modifications of organic structures which we see around us? Is 'survival of the fittest' or preservation by natural selection of those variations best adapted for their surrounding conditions (the essentially Darwinian or still more essentially Wallacian doctrine) the sole or even the chief of these agents? Can isolation, or the revived Lamarckian view of the direct action of the environment, or the effects of use or disuse accumulating through generations, either singly or combined, account for all? or is it necessary to invoke the aid of any of the numerous subsidiary methods of selection which have been suggested as factors in bringing about the great result?

Anyone who has closely followed these discussions, especially those bearing most directly upon what is generally regarded as the most important factor of evolution—natural selection, or 'survival of the fittest'—cannot fail to have noticed the appeal constantly made to the advantage, the utility, or otherwise of special organs or modifications of organs or structures to their possessors. Those who have convinced themselves of the universal application of the doctrine of natural selection hold that every particular structure or modification of structure must be of utility to the animal or plant in which it occurs, or to some ancestor of that animal or plant, otherwise it could not have come into existence; the only reservation being for cases which are explained by the principle which Darwin called 'correlation of growth.' Thus the extreme natural selectionists and the old-fashioned school of teleologists are so far in agreement.

On the other hand, it is held by some that numerous structures and modifications of structures are met with in nature which are manifestly useless; it is even confidently stated that there are many which are positively injurious to their possessor, and therefore could not possibly have resulted from the action of natural selection of favourable variations. Organs or modifications when in an incipient condition are especially quoted as bearing upon this difficulty. But here, it seems to me, we are continually appealing to a criterion by which to test our theories of which we know far too little, and this (though often relied upon as the strongest) is, in reality, the weakest point of the whole discussion.

Of the variations of the form and structure of organic bodies we are

beginning to know something. Our museums, when more complete and better organised, will teach us much on this branch of the subject. They will show us the infinite and wonderful and apparently capricious modifications of form, colour, and of texture to which every most minute portion of the organisation of the innumerable creatures which people the earth is subject. They will show us examples of marvellously complicated and delicate arrangements of organs and tissues in many of what we consider as almost the lowest and most imperfectly organised groups of beings with which we are acquainted. But as to the use of all these structures and modifications in the economy of the creatures that possess them, we know, I may almost say, nothing, and our museums will never teach us these things. If time permitted I might give numerous examples in the most familiar of all animals, whose habits and actions are matters of daily observation, with whose life-history we are as well acquainted almost as we are of our own, of structures the purposes of which are still most doubtful. There are many such even in the composition of our own bodies. How, then, can we expect to answer such questions when they relate to animals known to us only by dead specimens, or by the most transient glimpses of the living in a state of nature, or when kept under the most unnatural conditions in confinement? And yet this is actually the state of our knowledge of the vast majority of the myriads of living beings which inhabit the earth. How can we, with our limited powers of observation and limited capacity of imagination, venture to pronounce an opinion as to the fitness or unfitness for its complex surroundings of some peculiar modification of structure found in some strange animal dredged up from the abysses of the ocean, or which passes its life in the dim seclusion of some tropical forest, and into the essential conditions of whose existence we have at present no possible means of putting ourselves in any sort of relation?

How true it is that, as Sir John Lubbock says, 'we find in animals complex organs of sense richly supplied with nerves, but the functions of which we are as yet powerless to explain. There may be fifty other senses as different from ours as sound is from sight; and even within the boundaries of our own senses there may be endless sounds which we cannot hear, and colours as different as red from green of which we have no conception. These and a thousand other questions remain for solution. The familiar world which surrounds us may be a totally different place to other animals. To them it may be full of music which we cannot hear, of colour which we cannot see, of sensations which we cannot conceive.'

The fact is that nearly all attempts to assign purposes to the varied structures of animals are the merest guesses and assumptions. The writers on natural history of the early part of the present century, who 'for every why must have a wherefore,' abound in these guesses, which wider knowledge shows to be untenable. Many of the arguments for or against natural selection, based upon the assumed utility or equally

assumed uselessness of animal and vegetable structures, have nothing more to recommend them. In fact, to say that any part of the organisation of an animal or plant, or any habit or instinct with which it is endowed, is useless, or even injurious, seems to me an assumption which, in our present state of knowledge, we are not warranted in making. The time may come when we shall have more light, but infinite patience and infinite labour are required before we shall be in a position to speak dogmatically on these mysteries of nature—labour not only in museums, laboratories, and dissecting-rooms, but in the homes and haunts of the animals themselves, watching and noting their ways amid their natural surroundings, by which means alone we can endeavour to penetrate the secrets of their life-history. But until that time comes, though we may not be quite tempted to echo the despairing cry of the poet, ‘Behold, we know not anything,’ a frank confession of ignorance is the most straightforward, indeed the only honest position we can assume when questioned on these subjects.

However much we may be convinced of the supreme value of scientific methods of observation and of reasoning, both as mental training of the individual and in the elucidation of truth and advancement of knowledge generally, it is impossible to be blind to the fact that we who are engaged with the investigation of those subjects which are commonly accepted as belonging to the domain of physical science are unfortunately not always, by virtue of being so occupied, possessed of that most precious gift, ‘a right judgment in all things.’

No one intimately acquainted with the laborious and wavering steps of scientific progress (I can answer at least for one branch of it) can look upon that progress with a perfect feeling of satisfaction.

Can it be said of any of us that our observations are always accurate, the materials on which they are based always sufficient, our reasoning always sound, our conclusions always legitimate? Is there any subject, however limited, of which our knowledge can be said to have reached finality?

Or if it happens to any of us as to

A man who looks at glass
On it may stay his eye,
Or if he pleases through it pass
And then the heavens espy,

are not those heavens which are beyond the immediate objects of our observation coloured by our prejudices, prepossessions, emotions, or imagination, as often as they are defined by any profound insight into the depth of nature’s laws? In most of these questions an open mind and a suspended judgment appear to me the true scientific position, whichever way our inclinations may lead us.

For myself, I must own that when I endeavour to look beyond the glass, and frame some idea of the plan upon which all the diversity in the

organic world has been brought about, I see the strongest grounds for the belief, difficult as it sometimes is in the face of the strange, incomprehensible, apparent defects in structure, and the far stranger, weird, ruthless savagery of habit, often brought to light by the study of the ways of living creatures, that natural selection, or survival of the fittest, has, among other agencies, played a most important part in the production of the present condition of the organic world, and that it is a universally acting and beneficent force continually tending towards the perfection of the individual, of the race, and of the whole living world.

I can even go further and allow my dream still thus to run :

Oh yet we trust that somehow good
Will be the final goal of ill,—

That nothing walks with aimless feet,
That not one life shall be destroyed
Or cast as rubbish to the void
When God hath made the pile complete.

British Association for the Advancement of Science.

LEEDS, 1890.

ADDRESS

BY

SIR FREDERICK AUGUSTUS ABEL,

C.B., D.C.L. (Oxon.), D.Sc. (Cant.), F.R.S., P.P.C.S., Hon.M.Inst.C.E.

PRESIDENT.

MANY who had the pleasure of listening last year, at Newcastle, to the interesting and instructive Address of the President to whom I am a most ~~unworthy~~ successor, could not fail, both by the chief subject of his discourse and by the circumstance of the official position which he occupies with so much benefit to science and the public, to have their thoughts directed to the illustrious naturalist whose philosophical Address delighted the members of the Association and the people of Leeds thirty-two years ago.

More than one-half the period of existence of this Association has passed since Richard Owen presided over its meeting in this town. Alas! what gaps have been created in the ranks of those who then were prominent for activity in advancing its work: the then General Secretary, Sir Edward Sabine; the all-popular Assistant-general Secretary, John Phillips; the Treasurer, John Taylor, now live with us only through their works and the enduring esteem which they inspired. But very few of those who held other prominent positions at that meeting have survived to see the Association reassemble in this town. Whewell, Herschel, Hopkins, the elder Brodie, Murchison, William Fairbairn, all Presidents of Sections in 1858, have long since been removed from among us; and the then President of Section F, Edward Baines, a much-honoured and highly-talented son of the 'Franklin of Leeds,' whom we had hoped to count among those Vice-Presidents representing the City on this occasion, has recently passed away, in his ninetieth year, after a most honourable and useful career, during which he especially distinguished himself by his successful exertions in the advancement of the great educational movements of his time.

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The illustrious President of our last meeting here, concerning whose health the gravest apprehensions were not long since entertained, is happily still preserved to us; still intellectually bright at the ripe age of eighty-six, and still, with the keen pleasure of his early life, following the progress of those branches of scientific research which have constituted the favourite occupations and the arena of many intellectual triumphs of a long career of ardent and successful devotion to the advancement of science.

To not a few of those who have flocked to Leeds to attend the annual gathering of this Association, our present meeting-place is doubtless known chiefly by its proud position as one of the most thriving manufacturing towns of the United Kingdom; of ancient renown, especially in connection with one of the chief industries identified with Great Britain in years past. But this good town of Leeds, whose cloth market was described by Daniel Defoe, one hundred and sixty odd years ago, as 'a prodigy of its kind, and not to be equalled in the world,' and whose present position in connection with divers of our great industries would have equally excited the enthusiasm of that graphic writer, is famous for other things than its prominent association with manufactures and commerce.

Not many of our great industrial centres can boast of so goodly an array, upon the scroll of their past history, of names of men eminent in the Sciences, the Arts and Manufactures, in Divinity and Letters, and in heroic achievements, such as are identified with Leeds and its immediate vicinity: Thomas, Lord Fairfax, one of the most prominent heroes of the Commonwealth; Smeaton, an intellectual giant among engineers; William Hirst and John Marshall, illustrious examples of the men who by their genius, energy, and perseverance placed Great Britain upon the pinnacle of industrial and commercial greatness which she so long occupied unassailed; Richard Bentley, the eminent classic and divine; John Nicholson, the Airedale poet; John Fowler and Peter Fairbairn, worthy followers in the footsteps of Smeaton; Isaac Milner, weaver and mathematician, afterwards Senior Wrangler, Smith's prizeman, Jacksonian Professor, President of Queen's College, Vice-Chancellor of Cambridge University, Dean of Carlisle, and a most illustrious Fellow of the Royal Society; Thoresby, antiquarian and topographer; Benjamin Wilson, painter, and industrious contributor to the development of electrical science; William Hey, the eminent surgeon, and friend and counsellor of Priestley; Sadler, political economist and philanthropist; the brothers Sheepshanks—Richard, the astronomer, and John, the accomplished patron of the arts, and munificent contributor to our national art treasures; Edward Baines, whose conspicuous talents and energy developed a small provincial journal into one of the most powerful public organs of the country; his talented sons, of whom not the least conspicuous and highly respected was the late Sir Edward Baines. I might swell this voluminous list by reference to illustrious members of such families as that of Denison,

of Beckett, of Lowther, but the men I have referred to fitly illustrate the remarkable array of worthies whose careers have shed lustre upon the town in or near which they were born. Yet that illustration would be altogether incomplete if I failed to speak of one whose career and works alone would suffice to place Leeds in the foremost rank of those English towns which can claim as their own men whose course of life and whose achievements have secured their pre-eminence among our illustrious countrymen. Needless to say that I refer to Joseph Priestley, born within six miles of Leeds, whose name holds rank among the foremost of successful workers in science; who, by brilliant powers of experimental investigation, rapidly achieved a series of discoveries which helped largely to dispel the shroud of mystery surrounding the art of alchemy, and to lay the foundation of true chemical science. An ardent student of the classics, of Eastern languages, and of divinity, a zealous exponent of theological doctrines which marred his career as divine and instructor, he early displayed conspicuous talents for the cultivation of experimental science, which he pursued with ardour under formidable difficulties. His acquaintance with Franklin probably developed the taste for the study of electric science which led him to labour successfully in this direction; and the publication, in 1767, of his valuable work on 'The History and Present State of Electricity, with Original Experiments,' secured him a prominent position among the working Fellows of the Royal Society. His connection with Mill Hill Chapel, in 1768, appears to have given rise accidentally to his first embracing the experimental pursuit of what formerly was termed pneumatic chemistry, the foundation to which had been laid by Cavendish's memorable contribution, in 1766, to the 'Philosophical Transactions,' on carbonic acid and hydrogen. Priestley's first publication in pneumatic chemistry, on 'Impregnating Water with Fixed Air' (carbonic acid), attracted great attention; it was at once translated into French, and the College of Physicians addressed the Lords of the Treasury thereon, pointing out the advantages which might result from the employment, by men at sea, of water impregnated with carbonic acid gas, as a protective against, or cure for, scurvy.

Six years later Priestley investigated the chemical effects produced on the air by the burning of candles and the respiration of animals, and, having demonstrated that it was thereby diminished in volume and deteriorated, he showed that living plants possessed the power of rendering air, which had been thus deteriorated, once more capable of supporting the combustion of a candle. At about this time Priestley received very advantageous proposals to accompany Captain Cook upon his second expedition to the South Seas; but when about to prepare for his departure he learned from Sir Joseph Banks that objections against his appointment, on account of the great latitude of his religious principles, had been successfully urged by some ecclesiastic member of the Board of Longitude. In 1773 the Royal Society awarded

Priestley the Copley Medal for a remarkable paper entitled 'Observations on Different Kinds of Air,' and in that year he became librarian and literary companion to the Earl of Shelbourne (afterwards Marquis of Lansdowne), and thereby secured special advantages in the pursuit of his scientific researches.

With respect to his departure from Leeds, he expressed himself as having been very happy there 'with a liberal, friendly, and harmonious congregation, to whom my services (of which I was not sparing) were very acceptable. Here I had no unreasonable prejudices to contend with, so that I had full scope for every kind of exertion; and I can truly say that I always considered the office of a Christian minister as the most honourable of any upon earth, and in the studies proper to it I always took the greatest pleasure.' During the next five years he published as many volumes describing the results of important experiments on air. After investigating the properties of nitric oxide, and applying it to the analysis of air, Priestley, in 1774, discovered and carefully studied oxygen, which he obtained by the action of heat upon the red oxide of mercury. He was the first to prepare and study sulphurous acid, carbonic oxide, nitrous oxide, hydrochloric acid (*marine acid air*), and the fluoride of silicon, and carried out important researches on the properties of hydrogen, and of other gases previously but little known. His great quickness of perception and power of experiment led him to the achievement of many novel and important results; but one cannot help contrasting his somewhat random search after new discoveries with the close logical reasoning and philosophic spirit which guided and pervaded the remarkable researches of him whose departure from amongst us since the last gathering of this Association is so universally deplored—of the great discoverer of the universal law of the conversion of energy, James Prescott Joule. I could not add to the judicious and graceful reference to his work which Sir Henry Roscoe was privileged to make, in the last year of that philosopher's valuable life, when presiding over the recent meeting of the Association in the town which gloried in numbering Joule among its citizens; but I may, perhaps, be permitted to express the sanguine hope that the desire of the scientific world to secure the establishment of an international memorial fitly commemorative of his great life-work may be realised in the most ample manner.

The wide scope of the admirable discourse delivered by Owen in this town thirty-two years ago affords an interesting illustration of the delight which men whose best energies are devoted to the cultivation of one particular branch of science take in the results of the labours of their fellow-workers in other departments, and in their achievements in contributing to the general advancement of our knowledge of Nature's laws and of their operations. It is to this bond of intimate union between all workers in pure science that we owe the instructive reviews of the

progress made in different departments of science, with which we have often been presented at our annual gatherings. On the other hand, those men, from time to time selected to fill the distinguished office of President, whose lives have been mainly devoted to the practical utilisation of the results of scientific research, and to the extension in particular directions of the consequent resources of civilisation, seize with keen pleasure the opportunity afforded them of directing attention to the triumphs achieved in the application, to the purposes of daily life, of the great scientific truths established by such illustrious labourers in the fields of pure science as Newton, Dalton, Faraday, and Joule. The wide and constantly-extending domain of applied science presents, even to the superficial observer, a continually varied scene; not a year passes but some great prize falls to the lot of one or other of its explorers, and some apparently insignificant vein of treasure, struck upon but a few years back, is rapidly opened out by cunning explorers, until it leads to a mine of vast wealth, from which branch out in many directions new sources of power and might.

Among the branches of science in the practical applications of which the greatest strides have been made since the Association met at Leeds in 1858, is electricity. That year witnessed the accomplishment of the first great step towards the establishment of electrical communication between Europe and America, by the laying of a telegraph-cable connecting Newfoundland with Valencia. Through this cable a message of thirty-one words was shortly afterwards transmitted in thirty-five minutes; an achievement which, though exciting great enthusiasm at the time, scarcely afforded promise of the succession of triumphs of ocean telegraphy which have since surpassed the wildest dreams of the pioneers in the realms of applied electricity.

The development of the electric telegraph constitutes a never-failing subject of the liveliest interest. The experiments made by Stephen Gray, in 1727, of transmitting electrical impulses through a wire 700 feet long; by Watson, twenty years afterwards, of transmitting frictional electricity through many thousand feet of wire, supported by a line of poles, on Shooter's Hill, in Kent; and by Franklin, who carried out a similar experiment at Philadelphia,—although they were followed by many other interesting and philosophical applications of frictional electricity to the transmission of signals—were not productive of really practical results. The work of Galvani and of Volta was more fruitful of an approach to practical telegraphy in the hands of Sümmering and of Coxe, while the researches of Oersted, of Ampère, of Sturgeon, and of Ohm, and especially the discoveries of volta-electric induction and magneto-electricity by Faraday, paved the way for the development of the electric telegraph as a practical reality by Cooke and Wheatstone in 1837. How remarkable the strides have been in the resources and powers of the telegraphist since that time is demonstrated by a few such facts as these: the first needle-instrument of Cooke and Wheatstone transmitted messages at the rate of

four words per minute, requiring five wires for that purpose; six messages are now conveyed by one single wire, at ten times that speed, and news is dispatched at the rate of 600 words per minute. Duplex working, which more than doubled the transmitting power of a submarine cable, was soon eclipsed by the application of Edison's quadruplex working, which has in its turn been surpassed by the multiplex system, whereby six messages may be sent independently, in either direction, on one wire. When last the British Association met in Leeds, submarine telegraphy had but just started into existence; thirty years later, the accomplished President of the Mechanical Section informed us, at our meeting at Bath, that 110,000 miles of cable had been laid by British ships, and that a fleet of nearly forty ships was occupied in various oceans in maintaining existing cables and laying new ones.

The important practical achievements by which most formidable difficulties have been surmounted, step by step, in the successive attainment of the marvellous results of our day, have exerted an influence upon the advancement, not merely of electrical science, but also of science generally and of its applications, fully equal to that which they have exercised upon the development of commerce and of the intercourse between the nations of the earth.

Thus, the laying of the earliest submarine cables, between 1851 and 1855, led Sir W. Thomson, in conference with Sir George Stokes, to work out the theory of signalling in such cables, by utilising the mathematical results arrived at by Fourier in his investigation of the propagation of heat-waves. The failure of the first Atlantic cable led to the survey of the bottom of the Atlantic, which was the forerunner of deep-sea explorations, culminating in the work of the 'Challenger' Expedition, and opening up new treasures of knowledge scarcely dreamt of when last the British Association met at Leeds. To the difficulties connected with the early attempts at submarine telegraphy, and the determination with which Thomson drove home the lessons learned, we owe the systematic investigations into the causes of the variations in resistance of copper-conductors, and the consequent improvements in the metallurgy of copper, which led to the realisation of the high standard of purity of metal essential for the efficient working of telegraphic systems, and also to the extensive utilisation of electricity in the production of pure copper. The rare combination of originality in powers of research and perspicuity in mathematical reasoning, with inventive and constructive genius, for which Thomson has so long been pre-eminent, has placed at the disposal of the investigator of electric science, and of the practical electrician, instruments of measurement and record which have been of incalculable value, and which owe their origin to the theoretical conclusions arrived at by him in his researches into the conditions to be fulfilled for the attainment of practical success in the construction and employment of submarine cables. The mirror galvanometer, the quadrant electrometer, the

syphon-recorder, and the divided-ring electrometer, are illustrations of the valuable outcome of Thomson's labours; the combination of the last-named instrument with sliding resistance coils has rendered possible the accurate sub-division of a potential difference into 10,000 equal parts. The general use of condensers in connection with cable signalling, due to Varley's application of them for signalling through submerged cables with induced short waves, was instrumental in establishing the fact that all electro-static phenomena are simply the result of starting an electric current of known short duration round a closed circuit. The practical application of the Wheatstone Bridge led to numerous important mathematical investigations, and induced Clerk Maxwell to devise a new mode of applying determinants to the solution of the complicated electrical problems connected with networks of conductors. The necessity for the universal recognition of an electrical unit of resistance led to the establishment, in 1860, of the Electrical Standards Committee of the British Association, whose long succession of important annual reports was instrumental in most important developments of theoretical electricity, and, indeed, served to open up the whole science of electrical measurement. Matthiessen's important investigations of the electrical behaviour of metals and their alloys, and the preparation and properties of pure iron, were the outcome of the commercial demand for a practically useful standard of electrical resistance, while Latimer Clark's practical standard of electro-motive force, the mercurous sulphate cell, became invaluable to the worker in pure electrical research. The unit of resistance established by the British Association Committee received, in 1866, most important scientific application at the hands of Joule, who, by measuring the rate of development of heat in a wire of known resistance by the passage of a known current, obtained a new value of the mechanical equivalent of heat. This value differed by about 1.3 per cent. from the most accurate results arrived at by his experiments on mechanical friction, a difference which eventually proved to be exactly the error in the British Association unit of resistance; so that the true value of the unit of resistance, or Ohm was determined by Joule fifteen years before this result was achieved by electricians. Clerk Maxwell's remarkable electro-magnetic theory of light was put to the test, through the aid of the British Association unit of resistance, by Thomson, in determining the ratio of electro-magnetic unit to the electro-static unit of quantity. Many other most interesting illustrations might be given of the invaluable aid afforded to purely scientific research by the practical results of the development of electrical science, and of the constant co-operation between the science student and the practical worker. No one could, more fitly than the late Sir William Siemens, have maintained, as he did in his admirable Address at our meeting in Southampton, in 1882, that we owe most of the rapid progress of recent times to the man of science who partly devotes his energies to the solution of practical problems, and to the practitioner who finds relaxa-

tion in the prosecution of purely scientific inquiries. Most assuredly, both these classes of the world's benefactors may with equal right lay claim to rank the name of Siemens among those whom they count most illustrious!

In that highly interesting and valuable Address, delivered little more than a year before his sudden untimely removal from among us, the numerous important subjects discussed by him included not a few which he had made peculiarly his own in the wide range embraced by his enviable power of combining scientific research with practical work. Prominent among these were the applications of electric energy to lighting and heating purposes, and to the transmission of power, to the future development of which his personal labours very greatly contributed.

Siemens referred to the passing of the first Electric Lighting Bill, in the year of his Presidency, as being designed to facilitate the establishment of electric installations in towns; but the anxiety of the Government of that day to protect the interests of the public through local authorities, led to the assignment of such power to these over the property of lighting companies, that the utilisation of electric lighting was actually delayed for a time by those legislative measures. There can now be no doubt, however, that this delay has really been in the interests of intending suppliers and of users of the electric light, as having afforded time for the further development of practical details, connected with generation and distribution, which was vital to the attainment of a fair measure of initial success. The subsequent important modification of legislation on the subject of electric lighting, together with the practical realisation of comparatively economical methods of distribution, the establishment of fairly equitable arrangements between the public and the lighting companies, and the apportionment, so far as the metropolis is concerned, of distinct areas of operation to different competing companies, have combined to place electric lighting in this country at length upon some approach to a really sound footing, and to give the required impetus to its extensive development. Nine companies either are now, or will very shortly be, actually at work supplying, from central stations, districts of London comprising almost the entire western and north-western portions of the metropolis. As regards other parts of England, there are already twenty-seven lighting stations actually at work in different towns, besides others in course of establishment, and many more projected. The town of Leeds has not failed to give serious attention to the subject of utilising the electric light, and, although no general scheme has yet been adopted, the electricians who now visit this town will rejoice to see many of its public buildings provided with efficient electric illumination.

While the prediction made by Siemens, eight years ago, that electric lighting must take its place with us as a public illuminant, has thus been already, in a measure, fulfilled, important progress is being continuously made by the practical electrician in developing and perfecting the arrange-

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ments for the generation of the supply, its efficient distribution from centres, and its delivery to the consumer in a form in which it can be safely and conveniently dealt with and applied at an outlay which, even now, does not preclude a considerable section of the public from enjoying the decided advantages presented by electric lighting over illumination by coal-gas. Yet our recent progress in this direction, encouraging though it has been, is insignificant as compared with the strides made in the application of electric lighting in the United States, as may be gauged by the fact that, while in America the number of arc lamps in use, in April of this year, was 235,000, and of glow-lamps about three millions, there are at present about one-tenth the number of the latter, and one hundredth the number of arc lamps, in operation in England.

In some important directions we may, however, lay claim to rank foremost in the application of the electric light; thus, our large passenger-ships and our warships are provided with efficient electrical illumination; to the active operations of our Navy the electric light has become an indispensable adjunct; and our system of coast defence, by artillery and submarine mines, is equally dependent, for its thorough efficiency, upon the applications of electricity in connection with range-finding, with the arrangement and explosion of mines, and with the important auxiliary in attack and defence, the electric light, which, while so arranged, at the operating stations, as to be protected against destruction by artillery-fire and difficult of detection by the enemy, is available at any moment for affording invaluable information and important assistance and protection.

Other important applications of the electric light, such as its use as a lighthouse-illuminant, for the lighting of main roads in coal-mines, where its value is being increasingly appreciated, and even for signalling purposes in mid-air, through the agency of captive balloons, are continually affording fresh demonstrations of the value of this particular branch of applied electric science.

At the Electrical Exhibition at Vienna in 1883, where, not long before the lamented death of Siemens, I had the honour of serving as one of his colleagues in the representation of British interests, the progress which had been made in the construction of electrical measuring instruments since the French Exhibition and the Electrical Congress, two years before, was very considerable. The advance in this direction has been enormous since that time; but although the practical result of Thomson's and of Carlew's important work has been to supply us with trustworthy electrical balances and voltmeters, while efficient instruments have also been made by other well-known practical electricians, we have still to attain results in all respects satisfactory in these indispensable adjuncts to the commercial supply and utilisation of electric energy.

In connexion with this important subject the recent completion of the

Board of Trade standardising laboratory, established for the purposes of arriving at and maintaining the true values of electrical units, and of securing accuracy and uniformity in the manufacture of instruments supplied by the trade for electrical measurements, may be referred to with much satisfaction as a practical illustration of official recognition of the firm root which the domestic and industrial utilisation of electric energy has taken in this country.

The achievements of the telephone were referred to by Siemens in glowing terms eight years ago; but the results then attained were but indications of the direction in which telephonic inter-communication was destined speedily to become one of the most indispensable of present applications of electricity to the purposes of daily life. Preece, in speaking at Bath, two years ago, of the advances made in applied electricity, showed that the impediments to telephonic communication between great distances had been entirely overcome; and now, although considerably behind America and France in the use of the telephone, we are rapidly placing ourselves upon speaking terms with our friends throughout the United Kingdom. The operations of the National Telephone Company well illustrate our progress in telephonic intercommunication: that company has now 22,743 exchange lines, besides nearly 5,000 private lines, its exchanges number 272, and its call-offices 526. The number of instruments under rental in England has now reached 99,000; but, important as this figure is compared to our use of the telephone a very few years ago, it sinks into insignificance by the side of the number of instruments under rental in America, which at the beginning of the present year had reached 222,430, being an increase of 16,675 over the number in 1889. Only thirteen years have elapsed since the telephone was first exhibited as a practically workable apparatus to members of the British Association at the Plymouth Meeting, and the number of instruments now at work throughout the world may be estimated as considerably exceeding a million.

The successful transmission of the electric current, and the power of control now exercised over the character which electrically-transmitted energy is made to assume, are not alone illustrated by the efficiency of the arrangements already developed for the supply of the electric light from central stations. Siemens dwelt upon this subject at Southampton with the ardent interest of one who had made its development one of the objects of his energetic labours in later years, and also with a prophet's prognostications of its future importance. In speaking of the electric current as having entered the lists in competition with compressed air, the hydraulic accumulator, and the quick-running rope driven by water-power, Siemens pointed out that no further loss of power was involved in the transformation of electrical into mechanical energy than is due to friction, and to the heating of the conducting wires by the resistance they oppose, and showed that this loss,

calculated upon data arrived at by Dr. John Hopkinson and by himself, amounted at the outside to 38 per cent. of the total energy. Subsequent careful researches by the Brothers Hopkinson have demonstrated that the actual loss is now much less than it was computed at in 1885; as much as 87 per cent. of the total energy transmitted being realisable at a distance, provided there be no loss in the connecting leads used.

The Paris Electric Exhibition of 1881 already afforded interesting illustrations of the performance of a variety of work by power electrically transmitted, including a short line of railway constructed by the firm of Siemens, which was a further development of the successful result already attained in Berlin by Werner Siemens in the same direction, and was, in its turn, surpassed by the considerably longer line worked by Messrs. Siemens at the Vienna Exhibition two years later. Various short lines which have since then been established by the firm of Siemens are well known, and one of the latest public acts in the valuable life of Siemens was to assist at the opening of the electric tramway at Portrush, in the installation of which he took an active part, and where the idea, so firmly rooted in his mind from the date of his visit to the Falls of Niagara, in 1876, of utilising water-power for electrical transmission—a result first achieved on a small scale by Lord Armstrong—was more practically realised than had yet been the case. Since that time Ireland has witnessed a further application of electricity to traction purposes, and of water-power to the provision of the required energy, in the working of the Bessbrook and Newry tramway, while London at length possesses an electric railway, three miles in length, to be very shortly opened, which will connect the City with one of the southern suburbs through a tram subway, and, although including many sharp curves and steep gradients, will be capable of conveying one hundred passengers at a time, at speeds varying from thirteen to twenty-four miles per hour. During the past year a regular service of tramcars has been successfully worked, through the agency of secondary batteries, upon part of one of the large tramways of North London, with results which bid fair to lead to an extensive development of this system of working. The application of electricity to traction purposes has, however, received far more important development in the United States; at the commencement of this year there were in operation in different States 200 electrical tramroads, chiefly worked upon the Thomson-Houston and the Sprague systems, and having a collective length of 1,641 miles, with 2,346 motor-cars travelling thereon. Further extensions are being rapidly made; thus, one company alone has 39 additional roads, of a collective length of 385 miles, under construction, to be worked through the agency of storage-batteries.

The idea cherished by Siemens, and enlarged upon by him in more than one interesting address, of utilising the power of Niagara, appears about to be realised, at any rate in part; as a large tract of land has been recently acquired, by a powerful American Association, about a mile dis-

tant from the Falls, with a view to the erection of mills for utilising the power, which it is also proposed to transmit to distant towns; and an International Commission, with Sir William Thomson at its head, and with Mascart, Turrettini, Coleman Sellers, and Unwin as members, will carefully consider the problems involved in the execution of this grand scheme.

The application of electric traction to water-traffic, first successfully demonstrated in 1883, is receiving gradual development, as illustrated by the considerable number of pleasure-boats which may now be seen on the Upper Thames during the boating season, and in connection with which Professor George Forbes proposed, at our meeting last year, that stations for charging the requisite cells, through the agency of water-power, should be established at the many weirs along the river, so as to provide convenient electric coaling-stations for the river pleasure-fleet.

Electrically-transmitted energy was first applied in Germany to haulage work in mines by the firm of Siemens some years ago, and great progress has since been achieved herein on the Continent and in America. Comparatively little has been accomplished in this direction in England; but it is very interesting to note, on the present occasion, that the first successful practical application of electricity in this country to pumping and underground haulage-work was made in 1887, in this neighbourhood, at the St. John's Colliery, at Normanton, where an extensive installation, carried out by Mr. Immisch, so well known in connection with electric launches, is furnishing very satisfactory results in point of economy and efficiency. The gigantic installations existing for the same purposes in Nevada and California afford remarkable illustrations of the work to be accomplished in the future by electrically-transmitted energy.

Among the many subjects of importance studied by Joule with the originality and thoroughness characteristic of his work, was the application of voltaic electricity to the welding and fusion of metals. Thirty-four years ago he published a most suggestive paper on the subject, in which, after dealing with the difficulties attending the operation of welding, and of the interference of films of oxide, formed upon the highly heated iron surfaces, with the production of perfect welds either under the hammer or by the methods of pressure (of which he then predicted the application to large masses of forged iron), he refers to the possibility of applying the calorific agency of the electric current to the welding of metals, and describes an operation witnessed by him in the laboratory of his fellow-labourer, Thomson, of fusing together a bundle of iron wires by transmitting through them, when imbedded in charcoal, a powerful voltaic current. Joule afterwards succeeded in fusing together a number of iron wires with the current of a Daniell battery, and of welding together wires of brass and steel, platinum and iron, &c. In discussing the question of the amount of zinc consumed in a battery for

raising a given amount of iron to the temperature of fusion, he points out that the same object would probably be more economically attained by the use of a magneto-electric machine, which would allow the heat to be provided by the expenditure of mechanical force, developed in the first instance by the expenditure of heat; and he indicates the possibility of arranging machinery to produce electric currents which shall evolve one-tenth of the total heat due to the combustion of the coal used, so that 5,000 grains of coal applied through that agency would suffice for the fusion of one pound of iron. The successful practical realisation of Joule's predictions in regard to the application of electric currents, thus developed, to the welding of iron and steel, and to analogous operations, through the agency of the efficient machines devised by Professor Elihu Thomson, was demonstrated to the members of the Association by Professor Ayrton at Bath two years ago, and was shown upon a larger scale to visitors at the Paris Exhibition last year, and recently to highly interested audiences in London by our late President, Sir Frederick Bramwell. The latter demonstrated that the production of iron-welds by means of the Thomson machines was accomplished nearly twice as rapidly as by expert craftsmen; the perfection of the welds being proved by the fact that the strength of bars broken by tensile strains at the welds themselves was about 92 per cent. of the strength of the solid metal. At the Crewe Works Mr. Webb is successfully applying one of these machines to a variety of welding-work. The rapidity with which masses of metal of various dimensions are raised in those machines to welding heat is quite under control; the heat is applied without the advent of any impurities, as from fuel, and the speed of execution of the welding operation reduces to a minimum the time during which the heated surfaces are liable to oxidise. With such practical advantages as these, this system of electric welding bids fair to receive many useful applications.

Another very simple system of electric welding, especially applicable to thin iron- and steel-sheets, hoops, &c., has been contemporaneously elaborated in Russia by Dr. Bernados, and is already being extensively used. The required heat at the surfaces to be welded is developed by connecting the metal with the negative pole of the dynamo-machine, or of a battery of accumulators, the circuit being completed by applying a carbon electrode to the parts to be heated; the reducing power of the carbon is said to preserve the heated metal surfaces from oxidation during the very brief period of heating. This mode of operation appears to have been practised upon a small scale, some years ago, by Sir William Siemens, to whom we also owe the first attempt to practically apply electric energy to the smelting of metals.

In his Address in 1882 he referred to some results attained with his small electrical furnace, and pointed out that, although electric energy could, obviously, not compete economically with the direct combustion of fuel for the production of ordinary degrees of heat, the electric furnace

would probably receive advantageous application for the attainment of temperatures exceeding the limits (about 1800° C.) beyond which combustion was known to proceed very sluggishly. This prediction appears to have been already realised through the important labours of Messrs. Cowles, who some years ago attacked the subject of the application of electricity to the achievement of metallurgic operations with the characteristic vigour and fertility of resource of our Transatlantic brethren. After very promising preliminary experiments, they succeeded, in 1885, at Cleveland, Ohio, in maturing a method of operation for the production of aluminium-bronze, ferro-aluminium and silicium-bronze, with results so satisfactory as to lead to the erection of extensive works at Lockport, N.Y., where three dynamo-machines, each supplying a current of about 3,000 Ampères, are worked by water-power, through the agency of turbines, each of 500 h.p., eighteen electric furnaces being now in operation for the production of aluminium alloys. These achievements have led to the establishment of similar works in North Staffordshire, where a gigantic dynamo-machine has been erected, furnishing a current of 5,000 Ampères, with an E. M. F. of 50 to 60 volts. The arrangement of the electrodes in the furnaces, the preparation of the furnace-charges (consisting of mixtures of aluminium-ore with charcoal and with the particular granulated metal with which the aluminium is to become alloyed at the moment of its elimination from the ore); the appliances for securing safety in dealing with the current from the huge dynamo-machine, and many other details connected with this new system of metallurgic work, possess great interest. Various valuable copper- and aluminium-alloys are now produced by alloying copper itself with definite proportions of the copper-alloy, very rich in aluminium, which is the product of the electric furnace. The rapid production in large quantities of ferro-aluminium—which presents the aluminium in a form suitable for addition in definite proportions to fluid cast iron and steel—is another useful outcome of the practical development of the electric furnace by Messrs. Cowles.

The electric process of producing aluminium-alloys has, however, to compete commercially with their manufacture by adding to metals, or alloys, pure aluminium produced by processes based upon the method originally indicated by Oersted in 1824, successfully carried out by Wöhler three years later, and developed into a practical process by H. St. Claire Deville in 1854—namely, by eliminating aluminium from the double chloride of sodium and aluminium in the presence of a fluoride, through the agency of sodium. An analogous process, indicated in the first instance by H. Rose—namely, the corresponding action of sodium upon the mineral cryolite, a double fluoride of aluminium and sodium—has also been recently developed at Newcastle, where the first of these methods was applied, upon a somewhat considerable scale, in 1860, by Sir Lowthian Bell, but did not then become a commercial

success, mainly owing to the costliness of the requisite sodium. As the cost of this metal chiefly determines the price of the aluminium, technical chemists have devoted their best energies to the perfection and simplification of methods for its production, and the success which has culminated in the admirable Castner process constitutes one of the most interesting of recent illustrations of the progress made in technical chemistry, consequent upon the happy blending of chemical with mechanical science, through the labours of the chemical engineer.

Those who, like myself, remember how, between forty and fifty years ago, a few grains of sodium and potassium were treasured up by the chemist, and used with parsimonious care in an occasional lecture-experiment, cannot tire of feasting their eyes on the stores of sodium-ingots to be seen at Oldbury as the results of a rapidly and dexterously executed series of chemical and mechanical operations.

The reduction which has been effected in the cost of production of aluminium through this and other processes, and which has certainly not yet reached its limit, can scarcely fail to lead to applications of the valuable chemical and physical properties of this metal so widespread as to render it as indispensable in industries and the purposes of daily life as those well-known metals which may be termed domestic, even although, and, indeed, for the very reason that, its association with many of these, in small proportion only, may suffice to enhance their valuable properties or to impart to them novel characteristics.

The Swedish metallurgist, Wittenström, appears to have been the first to observe that the addition of small quantities of aluminium to fused steel and malleable iron had the effect of rendering them more fluid, and, by thus facilitating the escape of entangled gases, of ensuring the production of sound castings without any prejudicial effect upon the quality of the metal. The excellence of the so-called Mitis castings, produced in this way, appears thoroughly established, and the results of recent important experiments seem to be opening up a field for the extensive employment of aluminium in this direction, provided its cost becomes sufficiently reduced. The valuable scientific and practical experiments of W. J. Keep, James Riley, R. Hadfield, Stead, and other talented workers in this country and the United States, are rapidly extending our knowledge in regard to the real effects of aluminium upon steel, and their causes. Thus, it appears to be already established that the modifications in some of the physical properties of steel resulting from the addition of that metal, are not merely ascribable to its actual entrance into the composition of the steel, but are due, in part, to the de-oxidation by aluminium of some proportion of iron-oxide which exists distributed through the metal, and prejudicially affects its fluidity when melted. In the latter respect, therefore, the influence exerted by aluminium, when introduced in small proportions into malleable iron and steel, appears to be quite analogous to that of phosphorus, silicium, or lead

when these are added in small proportions to copper and certain of its alloys, the de-oxidation of which, through the agency of those substances, results in the production of sound castings of increased strength and uniformity. It is only when present in small proportion in the finished steel, that aluminium increases the breaking strain and elastic limit of the product.

The influence of aluminium, when used in small proportion, upon the properties of grey and white cast iron, is also of considerable interest, especially its effect in promoting the production of sound castings, and of modifying the character of white iron in a similar manner to silicium, causing the carbon to be separated in the graphitic form; with this difference—that the carbon appears to be held in solution until the moment of setting of the liquid metal, when it is instantaneously liberated, with the result that the structure of the cast metal and distribution of the graphite are perfectly uniform throughout.

The probable beneficial connection of aluminium with the industries of iron and steel naturally directs attention to the great practical importance, in the same direction, which has already been acquired, and promises to be in increasing measure attained, by certain other metals which, for long periods succeeding their discovery, have either been only of purely scientific interest and importance, or have acquired practical value in regard to their positions in a few directions quite unconnected with metallurgy. Thus, great interest attaches to the influence of the metals manganese, chromium, and tungsten upon the physical properties of steel and iron.

The name of Mushet is most prominently associated with the history of manganese in its relations to iron and steel. Half-a-century ago David Mushet carried out very instructive experiments on the influence exerted upon the properties of steel by the presence of manganese; and to Robert Mushet we owe the invaluable experiments leading to his suggestion to use manganese in the production of steel by the Bessemer process, which at once smoothed the path to the marvellously rapid and extensive development of the applications of steel produced by that classic method, and subsequently by the open-hearth or Siemens-Martin process—a development which has recently received its crowning illustration in the completion of one of the grandest of existing triumphs of engineering science and constructive skill—the Forth Bridge.

Robert Hadfield has recently contributed importantly to our knowledge of the relations of manganese to iron. His systematic study of the subject has revealed some very remarkable variations in the physical properties of so-called manganese-steel, according to the proportions of manganese which it contains. Thus, while the existence in steel of proportions ranging from 0.1 up to about 2.75 per cent. improves its strength and malleability, it becomes brittle if that limit is exceeded, the extreme of

brittleness being obtained with between 4 and 5 per cent. of manganese; if, however, the percentage is increased to not less than 7, and up to 20, alloys of remarkable strength and toughness are obtained. Castings of high manganese-steel, such as wheel-tyres, combine remarkable hardness with toughness. Even if the proportion of manganese is as high as 20 per cent. in a steel containing 2 per cent. of carbon, it can be forged; whereas it is very difficult to forge a steel of ordinary composition containing as much as 2.75 per cent. of carbon. Another remarkable peculiarity of the high manganese-steel is its behaviour when quenched in water. Instead of the heated metal being hardened and rendered brittle by the sudden cooling, like carbon-steel, its tensile strength and its toughness are increased; so that water-quenching is really a toughening process, as applied to the manganese-alloy; and an interesting feature connected with this is that, the colder the bath into which the highly-heated metal is plunged, the tougher is the product. The curious effect of manganese in reducing, and even destroying, the magnetic properties of iron was already noticed by Rinman nearly 120 years ago; one result of Hadfield's important labours has been to place in the hands of such eminent physicists as Thomson, John Hopkinson, and Reinold, materials for the attainment of most interesting information respecting the electrical and other physical characteristics of manganese-steel. Hopkinson, from experiments with a sample of steel containing 12 per cent. of manganese, estimated that not more than 9 out of the 86 per cent. of the iron composing the mass was magnetic, and he considered that the manganese enters into that which must, for magnetic purposes, be regarded as the molecule of iron, completely changing its properties, a fact which must have great significance in any theory regarding the nature of magnetisation. The great hardness of manganese-steel, and the consequent difficulty of dealing with it by means of cutting-tools, constitute at present the chief impediments to its technical applications in many directions; but where great accuracy of dimensions is not required, and where great strength is an essential, it is already put to valuable uses.

The importance of manganese in connection with the metallurgy of iron and steel is in a fair way of finding its rival in that of the metal chromium, the employment of which, as an alloy with steel, was first made the subject of experiment in 1821, by Berthier, who was led by the important experiments of Faraday and Stoddart, then just published, to endeavour to alloy chromium with steel, and obtained good results by fusing steel together with a rich alloy of chromium and iron, so as to introduce about 15 per cent. of the former into the metal. Further small experiments were made the year following, by Faraday and Stoddart, in the same direction; but chrome-steel appears to have been first produced commercially at Brooklyn, N.Y., sixteen years ago. Ten years later its manufacture had become developed in France, and the varieties of

chrome-steel produced in the Loire district now receive important and continually-extending applications, on account of their combining comparative hardness with high tenacity, and only little loss in ductility, and of their acquiring great closeness of structure when tempered.

The influence of chromium upon the character of steel differs in several marked respects from that exercised by manganese; thus, chrome-steels weld badly, or not at all, whereas manganese-steels weld very readily, and work under the hammer better than ordinary carbon-steel. Again, the remarkable influence of manganese upon the magnetic properties of steel and iron is not shared by chromium. Chrome-steel has for some time been a formidable rival of the very highest qualities of carbon-steel produced for cutting-tools, and of the valuable tungsten-steel which we owe to Robert Mushet. The great hardness, high tenacity and exceeding closeness of structure possessed by suitably-tempered steel containing not more than from 1 to 1.5 per cent. of chromium, and from 0.8 to 1 per cent. of carbon, renders this material invaluable for war purposes: cast projectiles, when suitably tempered, have penetrated compound steel and iron plates over 9 inches in thickness, such as are used upon armoured ships of war, without even sustaining any important change of form. The proper tempering of these projectiles necessitates their being produced hollow; their cavities or chambers are only of small capacity, but the charge of violent explosive which they can contain, and which can be set into action without the intervention of fuze or detonating appliance, suffices to tear these formidable punching-tools into fragments as they force their way irresistibly through the armoured side of a ship, and to violently project those fragments in all directions, with fearfully destructive effects. The employment of chromium as a constituent of steel plates used for the protection of ships of war is already being entered upon, and the influence exerted by the presence of that metal in small quantities in steel employed in the construction of guns is also at present a subject of investigation. At Crewe, Mr. F. Webb has for some time past used chromium, with considerable advantage, in the production of high-quality steels for railway requirements.

The practical results attained by the introduction of copper and of nickel as components of steel have also recently attracted much attention. At the celebrated French Steel Works of M. Schneider, at Creuzot, the addition of a small percentage of copper to steel used for armour-plates and projectiles is practised, with the object of imparting hardness to the metal without prejudice to its toughness. James Riley has found that the presence of aluminium in very small quantities facilitates the union of steel with a small proportion of copper, and that the latter increases the strength but does not improve the working qualities of steel. With nickel, Riley has obtained products analogous in many important respects to manganese steel; the remarkable differences in the physical

properties of the manganese alloys, according to their richness in that metal, are also shared by the nickel alloys, some of these being possessed of very valuable properties; thus, it has been shown by Riley that a particular variety of nickel-steel presents to the engineer the means of nearly doubling boiler-pressures, without increasing weight or dimensions. He has, moreover, found the co-existence of manganese in small quantity with nickel in the alloy to contribute importantly to the development of valuable physical properties.

The careful study of the alloys of aluminium, chromium, manganese, tungsten, copper, and nickel, with iron and with steel, so far as it has been carried, with especial reference to the influence which they respectively exercise upon the salient physical properties of those materials, even when present in them in only very small proportions, has demonstrated the importance of a more searching or complete application of chemical analysis, than hitherto practised, to the determination of the composition of the varieties of steel which practical experience has shown to be peculiarly adapted to particular uses. It appears, indeed, not improbable that certain properties of these, which have been ascribed to slight variations in the proportion or the condition of the constituent carbon, or in the amounts of silicium, phosphorus, and manganese which they contain, may sometimes have been due to the presence in minute quantities of one or other of such metals as those named, and to the effects which they produce, either directly, or indirectly by modifying or counteracting the effects of the normal constituents of steel. The important part now played by manganese in steel manufacture is an illustration of the comparatively recent results of research, and of practical work based on research, in these directions, and the effects of the presence in steel of only very small quantities of some of the other metals named are already, as I have pointed out, being similarly understood and utilised.

Such systematic researches as those upon which Osmond, Roberts-Austen and many other workers have been for some time past engaged, may make us acquainted with the laws which govern the modifications effected in the physical characteristics of metals by alloying these with small proportions of other metals. The suggestion of Roberts-Austen, that such modifications may have direct connection with the periodic law of Mendeleeff, which may furnish explanations of the causes of specific variations in the properties of iron and steel, has been followed up energetically by Osmond, who has experimentally investigated the thermal influence upon iron of the elements phosphorus, sulphur, arsenic, boron, silicium, nickel, manganese, chromium, copper, and tungsten. He regards his results as being quite confirmatory of the soundness of Roberts-Austen's suggestion, as they demonstrate that foreign elements having atomic volumes lower than iron tend to make it assume or preserve the particular molecular form in which it has itself the lowest atomic volume, while the converse is the case for the foreign elements of high atomic volume.

An analogous influence was found to be exerted by those two groups of elements upon the permanent magnetisation of steel.

Captivating as such deductions are, those who have devoted much attention to the practical investigation of iron, steel, and other metals, cannot but feel that much caution has to be exercised in drawing broad conclusions from the results of such researches as these. Like the investigations recently made with the object of ascertaining the condition in which carbon exists in steel, and the part played by it in determining the modifications in the properties developed in that material by the influence of temperature and of work done upon it, they are surrounded by formidable difficulties, arising from the practical impossibility of altogether eliminating the disturbing influences of minute quantities of foreign elementary bodies, co-existing in the metal operated upon, with those whose effects we desire to study. Certain it is, however, that by acquiring an accurate acquaintance with the composition of varieties of iron and steel exhibiting characteristic properties; by persevering in the all-important work of systematic practical examination of the mechanical and physical peculiarities developed in iron and steel of known composition by their association with one or more of the rarer metals in varied proportions, and by the further prosecution of chemical and physical research in such directions as those which have already been fruitful of most instructive results, such talented labourers as Chernoff, Osmond, Roberts-Austen, Barus and Stroudal, Hadfield, Keep, James Riley, Stead, Turner, and others, cannot fail to contribute continually to the development of improvements equalling in importance those already attained in the production, treatment, and methods of applying cast iron, malleable iron, and steel, or alloys equivalent to steel in their qualities.

The causes of the variations in the physical properties of steel produced by the so-called hardening, annealing, and tempering processes were for very many years a fruitful subject of experimental inquiry, as well as of theoretical speculation with regard to the condition in which the carbon is distributed in steel, according to whether the metal is hardened or annealed, or in an intermediate, tempered state. Recent researches have made our knowledge in the latter direction fairly precise; as yet, however, we are only on the track of definite information respecting the nature and extent of connection between the physical peculiarities of steel in those different conditions and the established differences in the form and manner in which the carbon is disseminated through it.

The careful systematic study of the modifications developed in certain physical properties of iron and steel by gradual changes of temperature between fusion of the metal and the normal temperature, has shown those modifications to be governed by a constant law, and that at certain critical temperatures special phenomena present themselves. This important subject, which was so clearly brought before the Association last year in the interesting lecture of Roberts-Austen, has been,

and is still being pursued by accomplished workers, among whom the most prominent is F. Osmond. The phenomenon of recalescence, or the re-glowing of, or liberation of heat in, iron and steel at certain stages during the cooling process, first noticed by Gore, and examined into by Barrett, appears to be the result of actual chemical combination between the metal and its contained carbon at the particular temperature attained at the time; while the absorption of heat, demonstrated by the arrest in rise of temperature during its continuous application to the metal, is ascribed to the elimination, within the mass, of carbon as an iron-carbide perfectly stable at low temperatures. The pursuit of a well-devised system of experimental inquiry into this subject has led Osmond to propound theories of the hardening and tempering of steel, which are at present receiving the careful study of physicists and chemists, and cannot fail to lead to further important advancement of our knowledge of the true nature of the influence of carbon upon the properties of iron.

Another important subject connected with the treatment of masses of steel, and with the influence exercised upon their physical characteristics by the processes of hardening and tempering, and by submitting them to oft-repeated concussions or vibrations, or frequent or long-continued strains, is the development and maintenance, or gradual disappearance, of internal stresses in the masses—one of the many important subjects to which attention was directed by Dr. Anderson, the Director-General of Ordnance Factories, in his very suggestive Address to the Mechanical Section last year. This question is one of especial interest to the constructor of steel guns, as the powers of endurance of these do not simply depend upon the quality of the material composing them, but are very largely influenced by the treatment which it receives at the hands of the gun-maker. Indeed, the highest importance attaches to the processes which are applied to the preliminary preparation of the individual parts of which the gun is constructed, and to the putting together of these so as to ensure their being and remaining in the physical condition best calculated to assist each other in securing for the structure the power of so successfully resisting the heavy strains to which it has to be subjected, as to suffer little alteration other than that due to the superficial action of the highly-heated products of explosion of the charges fired in the gun. The development of internal strains in objects of steel, especially by the hardening and tempering processes, or by their exposure to conditions favourable to unequal cooling of different parts of the mass, has long been a subject of much trouble and of experimental inquiry in connection with many applications of steel. Systematic experiments of the kind commenced, about eighteen years ago, by the late Russian general, Kalakoutsky, are now being pursued at Woolwich, with the objects of determining the nature and causes of internal stresses in steel gun-hoops and -tubes, and in shells, and of thereby establishing the proper course to be

adopted for avoiding, lessening, or counteracting injurious stresses, on the one hand, and for setting up stresses beneficial to the powers of endurance of guns, on the other. One method of experiment pursued, with parts of guns, is to cut narrow hoops off the forgings, after a particular treatment, which are then cut right across at one place, it being observed whether, and to what extent, the resulting gaps open or close. This important subject has also been similarly investigated by my talented old friend and fellow-worker, the President this year of the Mechanical Section, Captain Andrew Noble, whose name in connection with the science and practice of artillery is familiar to us as household words.

The Crimean War taught Nations many lessons of gravest import, to some of which Sir Richard Owen took occasion to call attention most impressively in the Address delivered here, before the miseries of that war had become past history. The development of sanitary science, to which he especially referred, and which sprang from the bitter experience of that sad epoch, has had its parallel in the development of the science of artillery; but it would indeed be difficult to establish any parallelism between the benefits which even the soldier and the sailor have reaped from the great strides made by both these sciences. The acquisition of knowledge of the causes of the then hopelessness of gallant struggles which medical skill and self-sacrificing devotion made against the sufferings of the victims of battles and of fell diseases, as deadly as the cruellest implements of war; the application of that knowledge to the provision of the blessings of antiseptic treatment of wounds and to the intelligent utilisation of disinfectants and of other valuable preventive measures, to the supply of wholesome water, of wholesome food in campaigning, of sensible clothing, and of wholesome air in hospitals, barracks, and ships—these are some few of the benefits which the soldier and the sailor have derived from the development of sanitary science, which was so powerfully stimulated by the terrible lessons learned during the long-drawn-out siege of Sebastopol; and it is indeed pleasant to reflect that there has been, for years past, most wholesome competition between Nations in the enlargement of those benefits, and their dissemination among the men whose vocation it is to slay and be slain. The periodical International Congresses on Hygiene and Demography, of which we shall cordially welcome next year's assemblage in London, and whose members will deplore the absence from among them of the veteran Nestor in the science and practice of hygiene, Sir Edwin Chadwick, have afforded conclusive demonstration of the heartiness with which Nations are now co-operating with a view to utilise the invaluable results attained by the successful labourers in sanitary science.

What, on the other hand, shall we say of the benefits which sailors and soldiers, in the pursuit of their calling, derive from the ceaseless costly competition amongst Nations for supremacy in the possession of for-

midable artillery, violent explosives, quick-firing arms of deadly accuracy, and fearful engines which, unseen, can work wholesale destruction in a fleet? And what can we say of the benefits acquired by individual Countries in return for their continuous, and sometimes ruinous, expenditure in endeavouring to maintain themselves upon an equality with their neighbours in gun-filling power? The conditions under which engagements by sea or land will in the future be fought have certainly become greatly modified from those of thirty-five years ago, and the duration of warfare, even between Nations in conflict who are on a fair equality of resources, must become reduced; but, as regards the results of a trial of strength between contending forces, similarly equipped, as they now will be, with the latest of modern appliances only varying in detail, these must, after all, depend, as of old, partly upon accident, favoured, perhaps, by a temporary superiority in equipment, partly upon the skill and military genius of individuals, and very much upon the characteristics of the men who fight the battles.

What really can be said in favour of the advances made in the appliances of war—and this is, perhaps, the view which in such a town as Leeds we should keep before our eyes to the exclusion of the dark side of the picture—is, that by continuous competition in the development of their magnitude, diversity, and perfection, the resources of the manufacturer, the chemist, the engineer, the electrician, are taxed to the uttermost, with the very important, although incidental, results, that industries are created or expanded and perfected, trades maintained and developed, and new achievements accomplished in applied science, which in time beneficially affect the advance of peaceful arts and manufactures. In these ways the expenditure of a large proportion of a country's resources upon material which is destroyed in creating destruction does substantially benefit communities, and tends to the accomplishment of such material progress by a Country as goes far to compensate its people for the sacrifices which they are called upon to incur for the maintenance of their dignity among Nations.

From this point of view, at any rate, it may interest members of the British Association for the Advancement of Science, and for the promotion of its applications to the welfare and happiness of mankind, to hear something of recent advances in one of the several branches of science in its applications to naval and military requirements with which, during a long and arduous official career, now approaching its close, I have become in some measure identified.

Since the Meeting of the Association in this town in 1858, the progress which has been made in the regulation of the explosive force of gunpowder, so as to adapt it to the safe development of very high energy in guns presenting great differences in regard to size and to the work which they have to perform, has been most important. The different forms of gunpowder which were applied to war-purposes in this and other countries,

until within the last few years, presented comparatively few differences in composition and methods of manufacture from each other, and from the gunpowder of our ancestors. The replacement of smoothbore guns by rifled artillery, which followed the Crimean War, and the great increase in the size and power of guns, necessitated by the application of armour to ships and forts, soon called, however, for the pursuit of investigations having for their object the attainment of means for variously modifying the action of fired gunpowder, so as to render it suitable for artillery of different calibres whose power could not be effectively, or, in some instances, safely, developed by the use of the only kind of gunpowder then employed in English artillery of all calibres.

The means resorted to in the earlier of these investigations, and adhered to for many years, for controlling the violence of explosion of gunpowder, consisted exclusively in modifying the size and form of the individual masses composing a charge, and of their density and hardness, with the object of varying the rate of burning of those masses in a gun; it being considered that, as the proportions of ingredients generally employed very nearly correspond to those required for the development of the greatest chemical energy by the thoroughly-incorporated materials, the attainment of the desired results should be, if possible, effected rather by modifications of the physical and mechanical characters of gunpowder, than by variations of the proportions and chemical characters of its ingredients.

The varieties of powder from time to time introduced into artillery-service, as the outcome of investigations in this direction, were of two distinct types: the first of these consisted of further developments of the old granulated or corned powder, being produced by breaking up more or less highly-pressed slabs of the material into grains, pebbles, or boulders of approximately uniform size and shape. Gunpowders of this class, ranging in size from about 1,000 pieces to the ounce to about 6 pieces to the pound, have performed efficient service, and certain of them are still employed. The character of the other type is based upon the theoretical view that uniformity in the action of a particular gunpowder, when employed under like conditions, demands not merely identity in regard to composition, but also identity in form, size, density, and structure of the individual masses of which a charge consists. To approach the practical realisation of this view, equal quantities of one and the same mixture of ingredients, presented in the form of powder of uniform fineness and dryness, must be submitted to a particular pressure, for a fixed period, in moulds of uniform size, the surrounding conditions and subsequent manufacturing processes being as nearly as possible alike. Practical experience has shown that uniformity in the ballistic properties of black powder can be even more readily secured by the thorough blending or mixing together of different products of manufacture, presenting some variations in regard to size, density, hardness, or other features, than

by aiming at an approach to identity in the characters of the individual grains or masses.

When our attention was first actively directed to the modification of the ballistic properties of powder, the subject had already been to some extent dealt with, in the United States, by Rodman and Doremus, and the latter had proposed the employment, in heavy guns, of charges consisting of large pellets of prismatic form. While this prismatic powder, which was first used in Russia, was being perfected, and extensively applied there as well as in Germany and England, the production of powder-masses more suitable, by the comparatively gradual nature of their explosion, for the very large charges required for the heavy artillery of the present day, was actively pursued in Italy, and by our own Government Committee on Explosives, the outcome of very exhaustive practical investigations being the very efficient Fossano powder, or *poudre progressif* of the Italians, and the boulder- and large cylindrical-powders produced at Waltham Abbey.

Researches carried out by Captain Noble and myself, some years ago, with a series of gunpowders, presenting considerable differences in composition, indicated that decided advantages might be secured, for heavy guns especially, by the employment of such a powder as would furnish a comparatively very large volume of gas, its explosion being at the same time attended by the development of much less heat than in the case of ordinary black powder. In the course of these researches much light was thrown upon the causes of the wearing or erosive action of powder-explosions upon the inner surface of the gun, an action which, especially in the larger calibres of artillery, produces so serious a deterioration of the arm that the velocity of projection and accuracy of shooting suffer considerably, the wear being especially great where the products of explosion, while under the maximum pressure, can escape between the projectile and the bore. The great velocity with which the very highly-heated gaseous and liquid (fused solid) products of explosion sweep over the heated surface of the metal, gives rise to a displacement of the particles composing the surface of the bore, which increases in extent as the latter becomes roughened, and thus opposes greater resistance; at the same time, the high temperature to which the surface is raised reduces the rigidity of the metal, and its consequent power of resisting the force of the gaseous torrent; and, lastly, some amount of chemical action upon the metal, by certain of the highly-heated, non-gaseous products of explosion, contributes towards an increase in the erosive effects. Experiments made upon a large scale by Captain Noble with powders of different composition, and with other explosives, have afforded decisive evidence that the explosive agent which furnishes the largest proportion of gaseous products, and the explosion of which is attended by the development of the smallest amount of heat, exerts least erosive action.

Some eminent German gunpowder-manufacturers, who were at this

time actively engaged upon the production of a suitable powder for heavy guns, directed their attention, not merely to an alteration of the proportions of the ingredients, but also to a modification in the character of charcoal employed; the eventual result was the production of a new prismatic powder, composed of saltpetre in somewhat higher proportion than in normal black powder, and of a very slightly-burned charcoal of reddish-brown colour, quite similar to the *charbon roux* which Violette produced about forty years ago for use in sporting-powder, by the action of superheated steam upon wood or other vegetable matter. This brown prismatic powder (or 'cocoa powder') differs from black powder not merely in colour: it burns very slowly in the open air, and in guns its action is comparatively gradual and long-sustained. The products of its explosion are simple; as the powder contains saltpetre in large proportion relatively to the sulphur and charcoal, these become fully oxidised, and a relatively very large amount of water-vapour is produced, partly because of the comparatively high proportion of water in the finished powder, and partly from the large amount of hydrogen in the slightly-charred wood or straw used. The smoke from a charge of brown powder differs but little in volume from that of black powder, but it disperses much more rapidly, owing to the speedy absorption of the finely-divided potassium salts, forming the smoke, by the large proportion of water-vapour through which they are distributed.

This kind of powder has been substituted, with considerable advantage, for black powder in guns of comparatively large calibre, but it soon became desirable to attain even more gradual action in the case of the very large charges required for guns of the heaviest calibres, such as the 110-ton gun, from which shot of about 1,800 lbs. weight are propelled by a powder-charge of 960 lbs. Brown powder has, therefore, been modified in composition to suit these conditions; while, on the other hand, a powder intermediate in rapidity of action between black powder and the brown prism powder has been found more suitable than the former for use in guns of moderately large calibre.

The importance which machine-guns and comparatively large, quick-firing guns have assumed in the armament of ships, has made it very desirable to provide a powder for them which will produce comparatively little or no smoke, as their efficient employment becomes greatly limited when, after a very few rounds rapidly fired, with black powder, the objects, against which it is desired to direct the fire are more or less completely hidden by the interposed smoke. Hence much attention has of late been directed to the production of smokeless, or nearly smokeless, powders for naval use. At the same time, the views of many military authorities regarding the importance of dispensing with smoke in engagements on land, have also created a demand for smokeless powders suitable for field-artillery and for small-arms.

The properties of ammonium-nitrate, of which the products of decom-

position by heat are, in addition to water-vapour, entirely gaseous, have rendered it a tempting material to those who have striven to produce a smokeless powder; but its deliquescent character has been a formidable obstacle to its application as a component of a useful explosive agent. By incorporating charcoal and saltpetre in particular proportions with ammonium-nitrate, F. Gaus recently claimed to have produced an explosive material free from the hygroscopic character common to other ammonium-nitrate mixtures, and furnishing only permanently gaseous and volatile, or smokeless, products of explosion. These anticipations were not realised, but they led the talented German powder-maker, Mr. Heide-mann, to produce an ammonium-nitrate powder possessing remarkable ballistic properties, and producing comparatively little smoke, which speedily disperses. It yields a very much larger volume of gas and water-vapour than either black or brown powder, and is considerably slower in action than the latter; the charge required to produce equal ballistic results is less, while the chamber-pressure developed is lower, and the pressures along the chase of the gun are higher, than with brown powder. No great tendency is exhibited by it to absorb moisture from an ordinarily dry, or even somewhat moist, atmosphere, but it rapidly absorbs water when the hygroscopic condition of the air approaches saturation, and this greatly restricts its use.

About five years ago reports began to reach us from France of the attainment of remarkable results with a smokeless powder employed with the repeating or magazine rifle then in course of adoption for military service, and of marvellous velocities obtained by the use of this powder, in specially constructed artillery of great length. As in the case of the explosive agent called *Mélinite*, the fabulously-destructive effects of which were much vaunted at about the same time, the secret of the nature of this smokeless powder was well preserved by the French authorities; it is now known, however, that more than one smokeless explosive has succeeded the original, and that the material at present in use with the Lebel repeating rifle belongs to a class of nitro-cellulose- or nitro-cotton preparations, of which several have been made the subject of patents in England, and of which varieties are also being used in Germany and other countries.

A comparison between the chemical changes attending the burning or explosion of gunpowder, and of the class of nitro-compounds represented by gun-cotton, at once explains the cause of the production of smoke by the former, and of the smokelessness of the latter. Whilst the products of explosion of the nitro compounds consist exclusively of gases and of water-vapour, gunpowder, being composed of a large proportion of saltpetre, or other metallic nitrate, mixed with charred vegetable matter and variable quantities of sulphur, furnishes products of which over 50 per cent. are not gaseous, even at high temperatures, and which are in part deposited as a fused solid—which constitutes the

fouling in a firearm—and in part distributed in an extremely fine state of division through the gases and vapours developed by the explosion, thus giving to these the appearance of smoke as they escape into the air.

So far as smokelessness is concerned, no material can surpass *gun-cotton* (or other varieties of nitro-cellulose); but, even if the rate of combustion of the fibrous explosive in a firearm could be controlled with certainty and uniformity, its application as a safe propulsive agent is attended by so many difficulties that the non-success of the numerous early attempts to apply it to that purpose is not surprising. Those attempts, commencing soon after the discovery of *gun-cotton*, in 1846, and continued many years later in Austria, consisted entirely in varying the density and mechanical condition of employment of the *gun-cotton* fibre. No difficulty was experienced in thus exercising complete control over the rapidity of burning in the open air; but when the material was strongly confined, as in the bore of a gun, such methods of regulating its explosive force were quite unreliable, as some slight unforeseen variation in its compactness or in the amount and disposition of the air-spaces in the mass, would develop very violent action. Much more promising results were subsequently obtained by me by reducing the fibre to a pulp, as in the ordinary process of making paper, and converting this into highly-compressed, homogeneous masses of the desired form and size. Some favourable results were obtained at Woolwich, in 1867–8, in field-guns, with cartridges built up of compressed *gun-cotton* variously formed and arranged, with the object of regulating the rapidity of explosion of the charge. But although comparatively small charges often gave high velocities of projection, without any indications of injury to the gun, the uniform fulfilment of the conditions essential to safety proved to be beyond absolute control, even in guns of small calibre; and military authorities not being, in those days, alive to the advantages which might accrue from the employment of an entirely smokeless explosive in artillery, experiments in this direction were not persevered in. At the same time, considerable success attended the production of *gun-cotton*-cartridges for sporting purposes, the rapidity of its explosion being controlled by various methods; very promising results were also attained with the Martini-Henry rifle and a lightly-compressed pulped *gun-cotton*-charge, of pellet-form, the uniform action of which was secured by simple means.

A nearly smokeless sporting-powder had, in the meantime, been produced by Colonel Schultze, of the Prussian Artillery, from finely-divided wood, converted after purification into a mildly explosive form of nitro-cellulose, and impregnated with a small portion of an oxidising agent. Subsequently this powder was produced in a granular form, and rendered considerably more uniform in character, and less hygroscopic; it then closely resembled the well-known E.C. sporting powder, which consists of a nitro-cotton reduced to pulp, incorporated with the nitrates

of potassium and barium, and converted into grains through the agency of a solvent and a binding material. Both these powders produce very little smoke compared with black powder, but do not compete with the latter in regard to accuracy of shooting, when used in military arms.

In past years both camphor and liquid solvents have been applied to the hardening of the surfaces of granulated or compressed masses of gun-cotton and of this class of its preparations, with a view to render them non-porous. In some smokeless powders of French, German, Belgian, and English manufacture, acetic ether and acetone have been also used, not merely to harden the granules or tablets of the explosive, but to convert the nitro-cellulose, in the first instance, into a more or less gelatinous condition, so that it can readily be incorporated with other components and rolled, or spread into sheets, or pressed into moulds, or squirted into wires, rods, or tubes, while still in a plastic state. When the solvent has afterwards been removed, the hardened, horn-like, or somewhat plastic product is cut up into tablets, or into strips or pieces of suitable dimensions, for conversion into charges or cartridges.

Another class of smokeless powder, similar in physical characteristics to these nitro-cellulose powders, but containing nitro-glycerine as an important component, has been originated by Mr. Alfred Nobel, the well-known inventor of dynamite, and bears resemblance in its physical characteristics to another of his inventions, called blasting-gelatine, one of the most interesting of known violent explosive agents. When one of the lower products of nitration of cellulose is impregnated with the liquid explosive, nitro-glycerine, it gradually loses its fibrous nature, becoming gelatinised while assimilating the liquid; and the resulting product almost possesses the characters of a compound. This preparation, and certain modifications of it, have acquired high importance as blasting-agents more powerful than dynamite, and possessed of the valuable property that their prolonged immersion in water does not separate from them any appreciable proportion of nitro-glycerine. The nitro-glycerine powder first produced by Mr. Nobel was almost perfectly smokeless and developed very high energy, accompanied by moderate pressures at the seat of the charge, but it possessed certain practical defects, which led to the development of several modifications of that explosive and various improvements in manufacture. The relative merits of this class of smokeless powder, and of various kinds of nitro-cellulose powder, are now under careful investigation in this and other countries, and several more or less formidable difficulties have been met with in their application, in small-arms especially; these arise in part from the comparatively great heat they develop, which increases the erosive effects of the products of explosion, and in part from the more or less complete absence of solid products. The surfaces of the barrel and of the projectile being left clean, after the firing, are in a condition favourable to their close adhesion while the bullet is propelled

along the bore, with the consequent establishment of very greatly increased friction. The latter difficulty has been surmounted by more than one expedient, but always at the cost of absolute smokelessness.

Our knowledge of the results obtained in France and Germany with the use of smokeless powders in the new rifles and in artillery is somewhat limited; our own experiments have demonstrated that satisfactory results are attainable with more than one variety of them, not only in the new repeating-arm of our infantry, but also with our machine-guns, with field-artillery, and with the quick-firing guns of larger calibre which constitute an important feature in the armament of our Navy. The importance of ensuring that the powder shall not be liable to undergo chemical change detrimental to its efficiency or safety, when stored in different localities where it may be subject to considerable variations of temperature (a condition especially essential in connection with our own Naval and Military service in all parts of the world), necessitates qualities not very easily secured in an explosive agent consisting mainly of the comparatively sensitive nitro-compounds to which the chemist is limited in the production of a smokeless powder. It is possible, therefore, that the extent of use of such a material in our ships, or in our tropical possessions, may have to be limited by the practicability of fulfilling certain special conditions essential to its storage without danger or possible deterioration. If, however, great advantages are likely to attend the employment of a smokeless explosive, at any rate for certain Services, it will be well worth while to adopt such special arrangements as may be required for securing these without incurring special dangers; this may prove to be especially necessary in our ships of war, where temperatures so high as to be prejudicial even to ordinary black powder, sometimes prevail in the magazines, consequent mainly upon the positions assigned to them in the ships, but which may be guarded against by measures not difficult of application.

The Press accounts of the wonderful performances of the first smokeless powder adopted by the French—which, it should be added, were in some respects confirmed by official reports of officers who had witnessed experiments at a considerable distance—engendered a belief that a very great revolution in the conduct of campaigns must result from the introduction of such powders. It was even reported very positively that noiselessness was one of the important attributes of a smokeless powder, and highly-coloured comparisons have, in consequence, been drawn in Service-periodicals, and even by some military authorities, between the battles of the past and those of the future: the terrific din caused by the firing of the many guns and the roar of infantry-fire, in heavy engagements, being supposed to be reduced to noise so slight that distant troops would fail to know in what direction their comrades were engaged, and that sentries and outposts would no longer be able to warn their comrades of the approaching foe by the discharge of their rifles. Military journals of

renown, misled by such legendary accounts, chiefly emanating from France, referred to the absence of noise and smoke in battles as greatly enhancing the demands for skill and courage, and as surrounding a fight with mystery. The absence of recoil when a rifle was fired with smokeless powder was another of the marvels reported to attend the use of these new agents of warfare. It need scarcely be said that a closer acquaintance with them has dispelled the credit given to such of the accounts of their supposed qualities as were mythical, and a belief in which could only be ascribable to a phenomenal combination of credulity with ignorance of the most elementary scientific knowledge.

The extensive use which has been made in Germany of smokeless or nearly smokeless powder in one or two special military displays has, however, afforded interesting indications of the actual change which is likely to be wrought in the conditions under which engagements on land will be fought in the future, provided these new explosives thoroughly establish and maintain their position as safe and reliable propelling agents. Although the powder adopted in Germany is not actually smokeless, the almost transparent film of smoke produced by independent rifle-firing is not visible at a distance of about 300 yards; at shorter distances it presents the appearance of a puff from a cigar. The most rapid salvo-firing by a large number of men does not have the effect of obscuring them from distant observers. When machine-guns and field-artillery are fired with the almost absolutely smokeless powder which we are employing, their position is not readily revealed to distant observers by the momentary vivid flash of flame and slight cloud of dust produced.

There now appears little doubt that in future warfare belligerents on both sides will alike be users of these new powders; the screening or obscuring effect of smoke will therefore be practically absent during engagements between contending forces, and while, on the one hand, the very important protection of smoke, and its sometimes equally important assistance in manœuvres, will thus be abolished, both combatants will, on the other hand, secure the advantages of accuracy of shooting and of the use of individual fire, through the medium of cover, with comparative immunity from detection. Such results as these cannot fail to affect, more or less radically, the principles and conditions under which battles have hitherto been fought. With respect to the Naval Service, it is especially for the quick-firing guns, so important for defensive purposes, that a smokeless powder has been anxiously looked for; by the adoption of such a powder as has during the past year been elaborated for our artillery, should experience establish its reliability under all Service conditions and its power to fulfil all reasonable requirements in regard to stability, these guns will not only be used by our ships under conditions most favourable to their efficiency, but their power will also be very importantly increased.

The ready and safe attainment of very high velocities of projection

through the agency of these new varieties of explosive agents, employed in guns of suitable construction, would appear at first sight to promise a very important advance in the power of artillery; the practical difficulties attending the utilisation of these results are, however, sufficiently formidable to place, at any rate at present, comparatively narrow limits upon our powers of availing ourselves of the advantages in ballistics which they may present. The strength of the gun-carriages and the character of the arrangements used for absorbing the force of recoil of the gun, need considerable modifications, not easy of application in some instances; greater strength and perfection of manufacture are imperative in the case of the hollow projectiles or shells to be used with charges of a propelling agent, by the firing of which in the gun they may be submitted to comparatively very severe concussions; the increased friction to which portions of the explosive contents of the shell are exposed by the more violent setting back of the mass may increase the possibility of their accidental ignition before the shell has been projected from the gun; the increase of concussion to which the fuze in the shell is exposed may give rise to a similar risk consequent upon an increased liability to a failure of the mechanical devices which are applied to prevent the igniting arrangement, designed to come into operation only upon the impact or graze of the projected shells, from being set into action prematurely by the shock of the discharge; lastly, the circumstance, that the rate of burning of the time-fuze which determines the efficiency of a projected shrapnel shell is materially altered by an increase in the velocity of flight of the shell, also presents a source of difficulty.

The fallibility of even the most simple forms of fuze, manufactured in very large numbers, although it may be remote, must always engender a feeling of insecurity, when shells are employed containing an explosive agent of the class which, in recent years, it has been sought, by every resource of ingenuity, combined with intimate knowledge of the properties of these explosives, to apply as substitutes for gunpowder in shells, on account of their comparatively great destructive power.

One of the first uses, for purposes of warfare, to which it was attempted to apply gun-cotton, was as a charge for shells. But even when this was highly-compressed, and accurately fitted the shell-chamber, with the intervention only of a soft packing between the surfaces of explosive and of metal, to guard against friction between the two upon the shock of the discharge, no security was attainable against the ignition of the comparatively sensitive explosive by friction established within its mass at the moment when the shell is first set in motion. By the premature explosion of a shell charged with gunpowder, no important injury is inflicted upon the gun, but a similar accidental ignition of a gun-cotton charge must almost inevitably burst the arm. The earlier attempts to apply gun-cotton as a bursting-charge for shells were several times attended by very disastrous accidents of this kind; but the fact, afterwards discovered,

that wet compressed gun-cotton, even when containing sufficient water to render it quite unflammable, can be detonated through the agency of a sufficiently powerful charge of fulminate of mercury, or of a small quantity of dry gun-cotton imbedded within it, has led to the perfectly safe application of gun-cotton in shells, provided the fuze, through the agency of which the initiative detonating agent in the shell comes into operation, is secure against any liability to premature ignition when the gun is fired. Many successful experiments have been made with shells thus charged with wet gun-cotton, which is now recognised as a formidable destructive agent applicable in shells with much less risk of casualty than attends the use of many other of the violent explosive bodies which it has become fashionable, in professional parlance, to designate as 'high explosives.'

Many devices and arrangements, more or less ingenious and complicated, have been schemed, especially in the United States, for applying preparations of the very sensitive liquid, nitro-glycerine, such as dynamite and blasting-gelatine, as charges for shells. Some of these consist in subdividing the charge by more or less elaborate methods; in others the shell is also lined with some soft elastic packing-material, and paddings of similar material are applied in the head and the base of the shell-chamber, with the object of reducing the friction and concussion to which the explosive is exposed when the projectile is first set in motion. Such arrangements obviously reduce the space available for the charge in the shell, and the best of them fail to render these explosives as safe to employ as wet gun-cotton. In order to avoid exposing shells loaded with such explosives to the concussion produced when propelling them by a powder-charge, compressed air has been applied as the propelling agent, and guns of special construction and very large dimensions, from which shells containing as much as 500 lbs. of gun-cotton or dynamite are projected through the agency of compressed air, have recently been elaborated in the United States, where great expectations are entertained of the value, for war-purposes, of these so-called pneumatic guns.

A highly ingenious device for utilising a class of very powerful explosives in shells, without any risk of accident to the gun, was not long since brought forward by Mr. Grösen, the well-known armour-plate and projectile manufacturer of Magdeburg. It consisted of a thoroughly efficient arrangement for applying the fact, first demonstrated by Dr. Sprengel, that mixtures of nitric acid of high specific gravity with solid or liquid hydrocarbons, or with the nitro-compounds of these, are susceptible of detonation, with development of very high energy. The two agents, of themselves non-explosive—nitric acid and the hydro-carbon, or its nitro product—are separately confined in the shell; when it is first set in motion by the firing of the gun, the fracture of the receptacle containing the liquid nitric acid is determined by a very simple device; the two

substances are then free to come into contact, and their very rapid mixture is promoted by the rotation of the shell, so that almost by the time that it is projected from the gun, its contents, at first quite harmless, have become converted into a powerfully explosive mixture, ready to come into operation through the action of the fuze. Although safety appears assured by this system, the comparatively complicated nature of the contrivance, and the loss of space in the shell thereby entailed, place it at a disadvantage, especially since some other very violent explosive agents have come to be applied with comparative safety in shells.

Between four and five years ago intelligence first reached us of marvellously destructive effects produced by shells charged with an explosive agent which the French Government was elaborating. The reported results surpassed any previously recorded in regard to violently destructive effects and great velocity of projection of the fragments of exploded shells, and it was asserted that the employment of this new material, Mélinite, was unattended by the usual dangers incident to this particular application of violent explosive agents, an assertion scarcely consistent with accounts which soon reached us of several terrible calamities due to the accidental explosion of shells loaded with Mélinite.

Although the secret of the precise nature of Mélinite has been extremely well preserved, it transpired ere long that extensive purchases were made in England, by or for the French authorities, of one of the many coal-tar derivatives which for some years past has been extensively manufactured for tinctorial purposes, but which, although not itself classed among explosive bodies until quite lately, had long before been known to furnish, with some metals, more or less highly explosive combinations, some of which have been applied to the production of preparations suggested as substitutes for gunpowder.

The product of destructive distillation of coal from which, by oxidation, this material is now manufactured, is the important and universally-known antiseptic and disinfectant, carbolic acid, or phenol. Originally designated carbazotic acid, the substance now known as picric acid was first obtained in small quantities as a chemical curiosity by the oxidation of silk, aloes, &c., and of the well-known blue dye indigo, which thus yielded another dye of a brilliant yellow colour. To the many who may regard this interesting phenol-derivative as a material concerning the stability and other properties of which we have little knowledge, it will be interesting to learn that it has been known to chemists for more than a century. It was first manufactured in England for tinctorial purposes by the oxidation of a yellow resin (*Xanthorrhœa hastilis*), known as Botany Bay gum. Its production from carbolic acid was developed in Manchester in 1862, and its application as a dye gradually extended, until, in 1886, nearly 100 tons were produced in England and Wales.

Although picric acid compounds were long since experimented with as explosive agents, it was not until a very serious accident occurred, in 1887,

at some works near Manchester where the dye had been for some time manufactured, that public attention was directed in England to the powerfully explosive nature of this substance itself. The French authorities appear, however, to have been at that time already engaged upon its application as an explosive for shells. It is now produced in very large quantities at several works in Great Britain, and it has been extensively exported during the last four years, evidently for other than the usual commercial purposes. Large supplies of phenol, or carbolic acid, have, at the same time, been purchased in England for France, and lately for Germany, doubtless for the manufacture of picric acid, very extensive works having been established for its production in both those countries. It has been made the subject of experiment by our military authorities, and its position has been well established as a thoroughly stable explosive agent, easily manufactured, comparatively safe to deal with, and very destructive when the conditions essential for its detonation are fulfilled.

The precise nature of Mélinite appears to be still only known to the French authorities: it is asserted to be a mixture of picric acid with some material imparting to it greater power; but accounts of accidents which have occurred even quite recently in the handling of shells charged with that material appear to show that, in point of safety or stability, it is decidedly inferior to simple picric acid. Reliable as the latter is, in this respect, its employment is, however, not unattended with the difficulties and risks which have to be encountered in the use, in shells, of other especially violent explosives. Future experience in actual warfare can alone determine decisively the relative value of violent explosive agents, like picric acid or wet gun-cotton, and of the comparatively slow explosive, gunpowder, for use in shells; it is certain, however, that the latter still presents distinct advantages in some directions, and that there is no present prospect of its being more than partially superseded as an explosive for shells.

With regard to submarine mines and locomotive torpedoes, such as those marvels of ingenuity and constructive skill, the Whitehead and Brennan torpedoes, the important progress recently made in the practical development of explosive agents has not resulted in the provision of a material which equals wet compressed gun-cotton in combining with great destructive power the all-important essential of safety to those who have to deal with these formidable weapons and to man the small vessels which have to perform the very hazardous service of attacking ships of war at short distances by means of locomotive torpedoes.

Although the subject of the development of explosive force for purposes of war has of late received from workers in applied science, from seekers of patentable inventions, and even from the public generally, a somewhat predominating share of attention, considering that we congratulate ourselves upon the enjoyment of a period of profound peace, yet the produc-

tion of new explosive agents for mining and quarrying purposes, which present or lay claim to points of superiority over the well-established blasting-agents, has been by no means at a standstill. For many years the main object sought to be achieved in this direction was to surpass, in power or adaptability to particular classes of work, the well-known preparations of nitro-glycerine and gun-cotton, which, during the past twenty years, have been formidable competitors and, in many directions, absolutely successful rivals of black powder. It is both interesting and satisfactory to note, however, that this object has of late, and especially since the publication of the results of labours of English and foreign Commissions on the causes of mine-accidents, been prominently associated with endeavours to solve the important problems of combining, in an explosive agent, efficiency in point of power with comparative non-sensitiveness to explosion by friction or percussion, and of securing its effective operation with little or no accompaniment of projected flame. Safety-dynamites, flameless explosives, water-cartridges, and other classes of materials and devices connected with the getting of coal, the quarrying of rock, or the blasting of minerals, have claimed the attention of those who guide the miner's work; in some of these directions the practical results obtained have been beyond question important, and, indeed, conclusive as regards the great diminution of risks to which men need be exposed in those coal-mines where the ordinary use of explosives, although not altogether inadmissible, may at times be attended with danger. It is to be feared that those results are still far from receiving the amount of application which might reasonably be hoped for; but, at any rate, there are, among the extensive mining districts where the employment of explosives in connection with the getting of coal cannot be dispensed with, several of importance where the use of gun-powder has almost entirely given place to the adoption of blasting-agents or methods of blasting, the employment of which is either not, or only very exceptionally, attended by the projection of flame or incandescent matter into the air where the shot is fired.

The mining public is especially indebted to German workers for much of the success which has been obtained in this direction, and also to the eminent French authorities, Mallard and Le Chatelier, for their thorough theoretical and practical investigations bearing upon the prevention of accidental ignition of fire-damp during blasting operations. Having arrived at the conclusion that fire-damp- and air-mixtures are not ignited by the firing of explosive preparations which develop by their detonation temperatures lower than 2220°C ., they found that ammonium-nitrate, although in itself susceptible of detonation, does not develop a higher temperature than 1130°C ., while the temperature of detonation of nitro-glycerine and gun-cotton are, respectively, 3170° and 2636° . The admixture of that salt with nitro-glycerine or gun-cotton in sufficient proportion to reduce the temperature of detonation to within safe limits allows, therefore, of the

employment of those explosive agents in the presence of fire-damp mixtures without risk of accident, and this fact has led to the effective use of such mixtures as safe blasting-agents in coal.

Those who have been content to labour long and arduously with the objects steadily in view of advancing our knowledge of the causes of mine-accidents and of developing resources and measures for removing or combating those causes, can cherish the conviction that recent improved legislation in connection with coal mines, based upon the results of those labours, has been already productive of decided benefits to the miner, even although it has fallen short of what might reasonably have been hoped for as an outcome of the very definite results and conclusions arrived at by the late Royal Commission on Accidents in Mines (in the recent much-lamented death of whose universally respected chairman, my late esteemed friend and colleague, Sir Warrington Smyth, the scientific world has sustained the loss of an ardent worker, and the miner, of an invaluable friend).

The fearful dangers arising from the accumulation of inflammable dust in coal-mines, and the equality of mine-dust with fire-damp in its direful power of propagating explosions, which may sometimes even be, in the first instance, established chiefly or entirely through its agency, have now been long recognised as beyond dispute; and it is satisfactory to know that permission to fire shots in mine-workings which are dry and dusty has, by recent legislation, been made conditional upon the previous laying of the dust by effective watering. In some mining districts, moreover, the purely voluntary practice has been extensively adopted by mine-owners of periodically watering the main roads in dry and dusty mines, or of frequently discharging water-spray into the air in such roads, which must tend greatly to reduce the possible magnitude of the disastrous results of a fire-damp- or dust-explosion in any part of the mine-workings.

The encouragement given to the application of the combined resources of ingenuity, mechanical skill, and knowledge of scientific principles, through the elaborate, but thoroughly practical comparative trials to which almost every variety of safety-lamp has, during the last few years, been submitted by competent and conscientious experimenters, has resulted in the provision of lamps to the hand of the miner which combine the essential qualities of safety, under the most exceptionally severe conditions, with good illuminating power, simplicity of construction, lightness, and moderate cost. Very important progress has also been made, since the first appointment of the late Accidents in Mines Commission, towards the provision of thoroughly serviceable and safe portable electric lamps for use in mines. Of those which have already been in the hands of the miners, several have fairly fulfilled his requirements as regards size, weight, and illuminating power of sufficient duration; but much still remains to be accomplished with respect to durability, simplicity, thorough portability, and cost, before the self-contained electric

lamp can be expected to compete successfully with the greatly improved miners' lamps which are now in use, or available.

The recent legislation in connection with mines is certainly deficient in any sufficiently decisive measure for excluding from mine-workings certain forms of lamps which, while fairly safe in the old days of sluggish ventilation, are unsafe in the rapid air-currents now frequently met with in mines; it is, however, very satisfactory to know that the strong representations on this subject, made by the late Commission, combined with force of example and with the conclusive demonstration of the superiority of other lamps, by exhaustive experiments, have led within the last two years to the very general abandonment of the unprotected Davy, Clanny, and Stephenson lamps in favour, either of simple, safe, modifications of these, or of other safe and efficient lamps, and that one possible element of danger to the miner has thus been eliminated, at any rate in many districts. In one important respect recent improved legislation has failed to effect a most desirable change, namely, in the substitution of safety-lamps for naked lights in workings where small local accumulations of fire-damp are discovered from time to time. There appears little doubt that one of the three fearful explosions which have occurred within the last twelve months—the explosion at Llanerch Colliery, near Pontypool—was caused by the continued employment of naked lights in a mine where inspection constantly revealed the presence of fire-damp. This explosion, and two other terrible disasters, at Mossfield Colliery, in Staffordshire, and at Morfa Colliery, near Swansea, which have occurred since the last meeting of the Association, may have seemed to weaken the belief that the operation of the recent Mines Regulation Act, which was based upon some of the results of seven years' arduous labour of the late Mines' Commission, must have resulted in very substantial improvement in the management of mines and in the conduct of work by the men. Happily, however, there is a consensus of opinion among those most competent to judge—*i.e.*, the Government Mine Inspectors—that very decided benefits have already accrued from the operation of the new Act. Although far from embodying all that the experienced mine-owners, miners, and scientific workers upon that Commission, as well as practical authorities in Parliament, concurred in regarding as reasonably adaptable, from the results of observation and experiment, to the furtherance of the safer working of mines, this Act does include measures, precautionary and preventive, of undeniable utility, well-calculated to lessen the dangers which surround the miner, and to add to his personal comfort underground. We may hope, moreover, that the operation of the Act is paving the way to more comprehensive legislation in the near future; for it can scarcely be doubted, by the light of recent sad experience, that there are directions in which both masters and men still hesitate to adopt, of their own free will, measures or regulations, methods of working or appliances and precautions, which are calculated to be important additional safeguards against

mine-accidents, and which are either left untouched, or only hesitatingly and imperfectly dealt with in the recent enactments.

My labours upon the late Mines' Commission represent only one of several subjects in connection with which it has been my good fortune to have opportunities of rendering some slight public service in directions contrasting with one of the main functions of my career, by endeavouring to apply the results of scientific research to a diminution of the risks to which particular classes of the community, or the public at large, are exposed—of being sufferers by explosions, the results of accidents or other causes.

During the pursuit of bread-winning vocations, and even in ordinary domestic life, the conditions, as well as the materials, requisite for determining more or less disastrous explosions are often ready to hand, and their activity may be evoked at any moment through individual heedlessness or through pure accident. Steam, or gases confined under pressure, volatile inflammable liquids, combustible gases, or finely-divided inflammable solids, are now all well recognised as capable of assuming the character of formidable explosive agents; but with respect to the three last-named, it is only of late that material progress has been made towards a popular comprehension and appreciation of the conditions conducive to danger, and of those, by the fulfilment of which, danger may be avoided. Thus, the causes of explosions in coal-laden ships, together with the occurrence of spontaneous ignition in coal-cargoes, another fruitful source of disaster, were made the subject of careful inquiry some years ago by a Royal Commission, upon which I had the pleasure of working with the late Dr. Percy, whose invaluable labours for the advancement of metallurgic science will always be gratefully remembered. The light thrown by that inquiry upon the causes of those disasters, and upon the conditions to be fulfilled for guarding against the accumulations of fire-damp, gradually escaping from occlusion in coal, and of heat, developed by chemical changes occurring in coal-cargoes, has unquestionably led to an important reduction of the risks to which coal-laden ships are exposed. Subsequent official inquiries and experimental investigations, in which I took part with the late Sir Warrington Smyth and other eminent naval officers, consequent upon the loss of H.M.S. 'Doterel' through the accidental ignition of an explosive mixture of petroleum spirit-vapour and air (and other calamities in warships originating with the gradual emission of fire-damp from coal), have resulted in the adoption of efficient arrangements for ventilating all spaces occupied by, and contiguous to, the large supplies of fuel which these vessels have to carry.

The thorough investigation, by Rankine and others, of the causes of explosions in flour-mills, which in years past were so frequent and disastrous, has secured the adoption of efficient measures for diminishing the production, and the dissemination through channels and other spaces

in the mills, of explosive mixtures of flour-dust and air, and for guarding against their accidental ignition. The numerous terrible accidents caused by the formation and accidental ignition of explosive mixtures of inflammable vapour and air in ships carrying cargoes of petroleum stored in barrels or in tanks, have, by the investigations to which they have given rise, led to the indication of effective precautionary measures for guarding against their recurrence. Again, the many distressing accidents, frequently fatal, which have attended the domestic use of those valuable illuminants, petroleum and mineral oils of kindred character, have been made the subject of exhaustive investigations, which have demonstrated that these disasters may readily be prevented by the employment of lamps of proper construction, and by the observance of very simple precautions by the users of them; and a recent official inquiry which I have conducted with Mr. Boverton Redwood has furnished most gratifying proof that very substantial progress has been made within the last few years by lamp-manufacturers in the voluntary adoption of such principles of construction as we had experimentally demonstrated to be essential for securing the safe use of mineral oils in lamps for lighting and heating purposes, the employment of which has, within a brief period, received enormous extension in this and other countries.

The creation and rapid development of the petroleum industry has, indeed, furnished one of the most remarkable illustrations which can be cited of industrial progress during the period which has elapsed since the British Association last met in Leeds. One year after that meeting, viz., on August 28, 1859, the first well, drilled in the United States with the object of obtaining petroleum, was successfully completed, and the rate of increase in production in the Pennsylvania oil-fields during the succeeding years is shown by the following figures:—

In 1859, 5,000 barrels (of forty-two American gallons) were produced. In the following year the production increased to 500,000 barrels; while in the next year (1861) it exceeded 2,000,000 barrels, at which figure it remained, with slight fluctuations, until 1865. The supply then continued to increase gradually, until, in 1870, it reached nearly 6,000,000 barrels; while in 1874 it amounted to nearly 11,000,000 barrels. In 1880 it amounted to over 26,000,000 barrels, and in 1882 it reached 31,000,000. Since then the supply furnished by the United States has fallen somewhat, and last year it amounted to 21,500,000 barrels. The production of crude petroleum in the Pennsylvanian fields, large as it has been, has not, however, kept pace with the consumption, for we find that the accumulated stocks, which on December 31, 1888, amounted to over 18,000,000 barrels, had become reduced to about 11,000,000 barrels at the close of last year. At this rate the surplus stock above ground will have vanished by the end of the current year. In addition to the petroleum raised in Pennsylvania, there is now a very large production in the State of Ohio; but this has not as yet been em-

ployed as a source of lamp-oil ; it is, however, transported by pipe line in great quantities to Chicago, for use as liquid fuel in industrial operations.

A few years after the development of the United States petroleum-industry, the production of crude petroleum in Russia also began to extend very rapidly. For more than 2,500 years Baku, on the borders of the Caspian Sea, has been celebrated for its naphtha springs and for the perpetual flames of the Fire Worshippers, fed by the marvellous subterranean supplies of natural gas. To a limited extent neighbouring nations appear to have availed themselves of the vast supplies of mineral oil at Baku during the past one thousand years. By the thirteenth century the export of the crude oil had already become somewhat extensive, but the production of petroleum from it by distillation is of comparatively recent date. In 1863 the supplies of petroleum from the Baku district amounted to 5,018 tons ; they increased to somewhat more than double during the succeeding five years. In 1869 and following three years the production reached about 27,000 tons annually, and in 1873 it was about 64,000 tons ; three years later, 153,000 tons were produced, and in the following five years there was a steady annual increase, until, in 1882, the production amounted to 677,269 tons ; in 1884 it considerably exceeded 1,000,000 tons, and last year it had reached the figure of about 3,300,000 tons. The consumption of crude petroleum as fuel for locomotive purposes has, moreover, now assumed very large proportions in Russia, and many millions of gallons are annually consumed in working the vast system of railways on both sides of the Caspian Sea.

The imported refined petroleum used in this country in lamps for lighting, heating, and cooking, was exclusively American until within the last few years, but a very large proportion of present supplies comes from Russia. The imports of kerosene into London and the chief ports of the United Kingdom during 1889 amounted to 1,116,205 barrels of United States oil, and 771,227 barrels of Russian oil. During the same period the out-turn of mineral oil for use in lamps by the Scottish Shale Oil Companies probably amounted to about 500,000 barrels.

Another important feature connected with the development of the petroleum industry is the great extent to which the less volatile products of its distillation have replaced vegetable and animal oils and fats for lubricating purposes in this and other countries. The value of petroleum as a liquid fuel and as a source of gas for illuminating purposes has, moreover, been long since recognised, and it is probable that one outcome of the attention which is now being given to the hitherto unworked deposits of petroleum in the East and West Indies, South America, and elsewhere, will be a very large increase in its application to these purposes. In the East Indies there are vast tracts of oil-fields in Burmah, Baluchistan, Assam, and the Punjab. The native Rangoon oil industry is one of great antiquity, although the oil was only used in the crude condition

until about thirty-five years ago, at which time Dr. Hugo Müller, with the late Warren De la Rue, whose many-sided labours and generous benefactions have so importantly contributed to the advancement of science, made valuable researches on the products furnished by crude oil imported from Rangoon. The resources of the oil-fields of Upper Burmah, especially of the district of Yenangyoung (or *creek of stinking water*), have since then been developed by British enterprise, and have attained to considerable importance since our annexation of Upper Burmah.

The great extension of the petroleum trade is gradually leading to very important improvements in the system of transport of the material over water and on land. Until recently this has been carried out entirely in barrels and tin cases; the consequent great loss from leakage and evaporation, accompanied by risk of accident, is now becoming much reduced by the rapidly-increasing employment of tank-steamers, which transport the oil in bulk. Tank railway-wagons have for some time past been in use in Russia, and there is prospect of these and of tank-barges being adopted here for the distribution of the oil; while in London, the practice is already spreading gradually of distributing supplies to tradesmen from tank road-wagons. Some considerable doubt as to whether the risk of accident has not rather been altered in character than actually reduced by the new system of transport, has not unnaturally been engendered in the public mind by the occurrence within a comparatively short period of several serious disasters during the discharge of cargoes from tank-vessels. The memorable explosion which took place in October, 1888, on board the 'Ville de Calais,' in Calais Harbour, with widespread, destructive effects, was followed by a similarly serious explosion in the 'Fergusons,' at Rouen, last December, and, more recently, by a fire of somewhat destructive character at Sunderland, resulting from the discharge into the river of petroleum-residues from a ship's tanks. In all these cases the petroleum was of a nature to allow inflammable vapour to escape readily from the liquid, so that an explosive mixture could be rapidly formed by its copious diffusion through the air. No similar casualty has been brought to notice as having happened to tank-ships carrying petroleum oil of which the volatility is in accordance with our legal requirements, and this points to the prudence of restricting the application of the tank system to the transport and distribution of such petroleum as complies with well-established conditions of safety.

Another most remarkable feature connected with the development of the petroleum industry is presented by the utilisation, within the last few years, of the vast supplies of natural inflammable gas furnished by the oil-fields.

In America this remarkable gas-supply was for a long time only used locally, but before the close of 1885 its conveyance to a distance by pipes, for illuminating and heating purposes, had assumed large proportions, one of the companies in Pittsburgh having alone laid 335

miles of pipes of various sizes, through which gas was supplied equivalent in heating value to 3,650,000 tons of coal per annum. Since then the consumption in and around Pittsburgh has probably been at least tripled. At the close of 1886 six different companies were conveying natural gas by pipes to Pittsburgh from 107 wells; 500 miles of pipe, ranging in diameter from 30 inches to 3 inches, were used by these companies, 232 miles of which were laid within Pittsburgh itself. The Philadelphia Company, the most important of these associations, then owned the gas-supply from 54,000 acres of land situated on all the anti-clinals around Pittsburgh, but drew its supplies only from Tarentum and the Murrysfield field. It supplied, in 1886, 470 factories and about 5,000 dwellings within the city, besides many factories and dwellings in Alleghany, and in numerous neighbouring villages. The average gas-pressure at the wells, when the escape is shut off, is about 500 lbs. per square inch, and in the case of new wells this pressure is very greatly exceeded. In order to minimise the danger from leakage, the gas-pressure in the city is reduced to a maximum of 13 lbs., and is regulated by valves at a number of stations under the control of a central station. The usual pressure in the larger lines is from 6 to 8 lbs., while in the low-pressure lines it does not exceed 4 to 5 ounces.

The effect of the change from coal gas to natural gas upon the atmosphere over Pittsburgh has been most marked: formerly the sky was constantly obscured by a canopy of dense smoke; now the atmosphere is clear, and even white paint may with impunity be employed for the house fronts.

The very rapid development of the employment of natural gas is not confined to the neighbourhood of Pittsburgh; it is used for heating purposes in the cities of Buffalo, Erie, Jamestown, Warren, Olean, Bradford, Oil City, Titusville, Meadville, Youngstown, and perhaps twenty more towns and villages in Pennsylvania and North-western New York. In North-western Ohio, the cities of Toledo and Sandusky, the towns of Findlay, Lima, Tiffin, Fostoria, and others in that section are also supplied with natural gas; a pipe line has moreover been recently laid to Detroit, Mich., and it is estimated that in these localities 36,131,669,000 cubic feet of the gas were consumed during last year, displacing 1,802,500 tons of coal. To the south-west of Pittsburgh there are many smaller places which consume natural gas; it also occurs in considerable quantity, and is being utilised, in Indiana (whence an account has recently reached us of a terrific subterranean explosion of the gas); and it is at the present time contemplated to carry a natural gas-supply to Chicago.

The utilisation of the natural gas of the Russian oil-fields, although of very ancient date, has hitherto not been extensive, neither does the magnitude of the supply appear to bear comparison with that of the Pennsylvanian district.

A form of gaseous fuel which has long been known to technical

chemists and metallurgists, but which has of late attracted considerable attention, especially in connection with the recent interesting work relating to its applications pursued by Mr. Samson Fox, of Leeds, has become, within the last four years, a competitor in the United States, both of the natural gas of Pennsylvania and of coal-gas. Since Felix Fontana first produced so-called water gas in 1780, by passing vapour of water over highly-heated fuel, many methods, differing chiefly in small details, have been proposed for carrying out the operation, with a view to the ready and cheap production of the resulting mixture of hydrogen and carbonic oxide, and numerous technical applications of water-gas have been suggested from time to time, with no very important results, excepting as regards its use for lighting-purposes. Being of itself non-luminous, its utilisation in this direction is accomplished, either by mixing it with a highly luminous gas, or by causing a hydrocarbon vapour to be diffused through it, or the non-luminous flame, produced by burning it in the air, is made to raise to incandescence some suitably prepared solid substance, such as magnesia, lime, a zirconium salt, or platinum, whereby bright light is emitted. The objection to its employment as an illuminant for use in buildings, to which great weight is attached by us, and rightly, as sad experience has shown—viz., that, as it consists, to the extent of about one-half its volume, of the highly poisonous gas carbonic oxide, the atmosphere in a confined space may be rendered irrespirable by a small accidental contamination with water-gas, by leakage or otherwise, not detectable by any odour—appears to constitute no great impediment to its employment in the United States, as it is now manufactured for illuminating and heating purposes by a large proportion of their gas-works, being in some places employed in admixture with a highly luminous coal-gas, in others rendered luminous by the alternative methods mentioned. It is stated that about three-fourths of the illuminating gas now supplied to the cities of New York, Brooklyn, Philadelphia, Jersey, St. Paul, and Minneapolis, is carburetted water-gas; in Chicago the entire supply now consists of this gas, and Boston will also soon be supplied exclusively with it. The use of water-gas for metallurgic work does not appear to be contemplated in the United States, but it is especially to such applications of the gas that much attention has been devoted here in Leeds; and although some eminent experts are sceptical regarding the attainment of advantages, especially from an economical point of view, by the employment of this form of gaseous fuel, especially after practical experience in the same direction acquired in Germany, the technical world must feel grateful to Mr. Fox for his work in this direction, affording, as it does, an interesting illustration of the qualities of perseverance and energy which, when combined with sound knowledge, often achieve success in directions that have long appeared most unpromising—qualities, which have been characteristic of many pioneers in industrial progress in this country.

Leeds has been especially fortunate in the possession of such pioneers

who, when competition brought about great changes in the particular trade through which, for many generations, this city chiefly enjoyed prosperity and high renown, developed its power and resources in new directions, from which success soon flowed in continually increasing measure. The rapid rise of Leeds to its present high position in industrial prosperity and national importance most probably dates from the period when its chief staple industry began to experience serious rivalry, in its own peculiar achievements, on the part of other districts of the kingdom and of other countries. From early days a flourishing Centre of one of the provinces of Great Britain most richly endowed with some of Nature's best treasures, Leeds could scarcely have failed, through the energy, acute intelligence, and powerful self-reliance especially characteristic of the men of Yorkshire, to rapidly acquire fresh renown in connection with industries which either were new to the town and district, or had been pursued in comparatively modest fashion, and which have combined to place the Leeds of to-day upon a higher pinnacle of commercial prosperity, power, and influence than her patriotic citizens of old could ever have dreamt of.

An examination into the present educational resources of Leeds places beyond any doubt the fact that her present prosperity in commerce and industries is in no small degree ascribable to the paramount importance long since attached here to the liberal provision of facilities for the diffusion of knowledge among the artisan- and industrial-classes, and especially for the acquisition of a sound acquaintance with the principles of the sciences and their applications to technical purposes, with particular reference to the prominent local industries, by all grades of those who pursue or intend to pursue them. There is, probably, no town in the kingdom more amply provided with efficient elementary and advanced schools for both sexes, while the special requirements of the artisan are efficiently met by the prosperous School of Science and Technology. The resources of the Yorkshire College provide, in addition, a combination of thorough scientific education with really practical training in the more important local industries; indeed, during the sixteen years of its continually-progressive work, this institution has acquired so widespread a reputation that students come from abroad to reap the advantages afforded by the unrivalled textile and dyeing departments of the Leeds College. The keen competition now existing between these departments and the corresponding branches of the much younger but most vigorous sister College at Bradford, can only conduce to the further development of both, and to their thorough maintenance up to the requirements of the day.

The very important pecuniary aid afforded to these establishments, and to a number of other technical schools in Yorkshire, by one of the most important of the ancient companies of the City of London, the Clothworkers, affords an interesting illustration of the good work in the cause of education performed by those Guilds and, especially of late years,

by means of their flourishing Institute for the advancement of technical education which, through its two great instructional establishments in London, and through the operation of its system of examinations throughout the country, extending now even to the Colonies, has afforded very important aid towards eradicating the one great blot upon our national educational organisation. To have been first in the field in practically developing a far-reaching scheme for the advancement of technical education in this country must continue to be a source of pride to the City of London and its ancient Guilds in time to come, when the operation of efficient legislation, supported and extended by patriotic munificence and by the hearty co-operation of associations of earnest and competent workers in the cause, shall have placed the machinery and resources for the technical instruction of the people upon a footing commensurate with our position among Nations.

The remarkable Address delivered by Owen here in 1858, wherein the condition, at that time, of those branches of natural science which he had made particularly his own was most comprehensively reviewed, included some especially interesting observations on the importance to the cultivation and progress of the natural sciences, and to the advancement of education of the masses in this country, of providing adequate space and resources for the proper development of our National Museum of Natural History; and it cannot but be a source of great satisfaction and pride to him to have lived to witness the thoroughly successful realisation of the objects of his own indefatigable strivings and powerful advocacy in that direction. Comprehensive as were the views adopted by Owen regarding the scope and possible extension of that museum, it may, however, be doubted whether they ever embraced so extensive a field as was presented for our contemplation by his successor last year, when he told us that a natural history museum should, in its widest and truest sense, represent, so far as they can be illustrated by museum-specimens, all the sciences which deal with natural phenomena, and that the difficulties of fitly illustrating them have probably alone excluded such subjects as astronomy, physics, chemistry, and physiology, from occupying departments in our National Museum of Natural History.

The application, in its broadest signification, of the title, Natural History Museum, may doubtless be considered to include, not only illustrations and examples of the marvellous works of the Creator and of the results of man's labours in tracing their intimate history and their relations to each other, but also illustrations of the means employed, and of the results attained, by man in his strivings to fathom and unravel the laws by which the domains of Nature are governed. But, the reason why representative collections, illustrative of the physical sciences, do not form part of our National Natural History Museum, has, I venture to think,

scarcely been correctly ascribable to any difficulty of organising fit illustrations of methods of investigation, of the attendant appliances, and of the results attained by experimental research; it appears rather, to exist in the fact that physical science has hitherto had no share in such a combination of circumstances as has been favourable to the good fortunes and advancement of the natural sciences, and is analogous to those which, from time to time, give rise to the provision of increased accommodation for our National Art Treasures. Our present National Science Collection, which has, indeed, had a struggle for existence, does not owe the development it has hitherto experienced to any such moral pressure as has been several times exercised in the case of our art collections, by the munificence of individuals, with the result of securing substantial aid from national resources; its gradual increase in importance has been due to the untiring perseverance of men of science, and of a few prominent influential and public-spirited authorities, in keeping before the public the lessons taught by careful inquiries, such as those entrusted to the Royal Commission on Scientific Instruction, into the opportunities afforded for the cultivation of science and the development of its applications, in other Countries, as compared with those provided here.

The success of the efforts made in 1875 by a committee thoroughly representative of every branch of experimental science, to bring together in London an international loan collection of scientific apparatus, and the widespread interest excited by that collection, led the President of the Royal Society, in union with many distinguished representatives of science, to lay before our Department of Education a proposal to establish a national museum of pure and applied science, including the Museum of Inventions, which had already existed since 1860 as a nucleus of a science-museum, the establishment whereof had formed part of the original scheme of the Science and Art Department. The Loan Collection of 1876 did, in fact, and in consequence of the urgent representations then made, first put into practical shape the long-cherished desire of men of science to see an Institution arise in England similar to the Conservatoire des Arts et Métiers of France, and it became the starting-point of the National Collection, representative of the several branches of experimental science, which has been undergoing slow but steady development since that time, patiently awaiting the provision of a suitable home for its contents. This collection, which illustrates not only the means whereby the triumphs of scientific research have been and are achieved, but also the methods by which these departments of science are taught, yields, small as it is, to none of our national museum-treasures in interest and importance.

In yet another way did that Loan Collection become illustrious: one of the most interesting features connected with it was the organisation of a series of important conferences and explanatory lectures, serving to

illustrate, and also greatly to enhance, its value, and affording most invaluable demonstration of the way in which such collections must exercise direct influence upon the advancement of science and upon the diffusion of scientific knowledge. These lectures and conferences demonstrated the wisdom of the suggestion made by the illustrious representative of associated Science in Leeds eighteen years previously, that public access to museums should be combined with the delivery of lectures emphasising and amplifying the information afforded by their contents. The example there set of thoroughly utilising for instructional purposes, and for the advancement of science, a collection illustrative of the physical sciences, has since been followed by the Science and Art Department; illustrative lectures connected with the existing nucleus of a national science-collection therewith have been delivered from time to time, and the objects in the collection are constantly utilised in the courses of instruction of the adjoining Normal School of Science.

Although the national importance of thoroughly representative and continuously-maintained science collections has long been manifest, not only to all workers in science, but also to all who have cared to inquire, even superficially, into the influence of the cultivation of science upon the industrial and commercial prosperity of the country, the labours of a Royal Commission, and of successive Committees, in demonstrating the necessity for the provision of adequate accommodation for such collections, and for their support upon the basis of that afforded to the natural history collections, have been very long in bearing fruit. However, lovers of science, and those who have the prosperity of the country near at heart, have at length cause for rejoicing at the acquisition by the Nation of a site in all respects suitable and adequate for the accommodation of the science collections, which, as soon as appropriate buildings are provided for their reception, will not fail, in comprehensiveness and completeness, to become worthy of a Country which has been the birthplace of many of the most important discoveries in science, and of a People who have led the van among all Nations in making the achievements of science subservient to the advancement of industries and commerce.

The site selected as the permanent home of our National Science Collections is immediately in rear of the Natural History Museum, and faces the stately edifice, now rapidly progressing towards completion, for the erection of which, as an Imperial memorial of the Queen's Jubilee, funds were provided by voluntary contributions from every portion of the Empire and every class in the Empire's Nations. The Imperial Institute, the conception of which we owe to His Royal Highness the Prince of Wales, occupies a central position among buildings devoted to the illustration and cultivation of pure and applied Science and of the Arts—*i.e.*, the Normal School of Science, the Technical College of the City and Guilds of London, the National Schools of Art, the Science Museum, the South Kensington Museum, and the Royal College

of Music; to which we may ere long see added a National Gallery of representative British Art. A more fitting location could scarcely be conceived for this pre-eminently National Institution, which has for its main objects the comprehensive and continuously progressive illustration: of the practical applications of the vast resources presented by the Animal, Vegetable, and Mineral Kingdoms to Industries and the Arts; of the extent and the progressive opening up of those resources in all parts of the Empire; of the practical achievements emanating from the results of scientific research; and of the utilisation of the Arts for the purposes of daily life. With the attainment of these objects it will be the function of the Imperial Institute to combine the continuous elaboration of systematic measures tending to stimulate progress in trades and handicrafts, and to foster a spirit of emulation among the artisan- and industrial-classes. Another branch of the Institute's work, upon which it is already engaged, is the systematic collection of data relating to the natural history, commercial geography, and resources of every part of the Empire, for wide dissemination together with all current information, bearing upon the commerce and industries of the Empire and of other Countries, which can be comprised under the head of Commercial Intelligence. The achievement of these objects should obviously tend to maintain intimate intercourse, relationship, and co-operation between the great Home- and Colonial centres of Commerce, Industries, and Education, and to enhance importantly our power of competing successfully in the great struggle, in which Nations are continuously engaged, for supremacy in commercial and industrial enterprise and prosperity.

To the elaboration of the practical details of a system of operation calculated to secure the objects I have indicated, eminent public-spirited men are now devoting their best energies, with the sanguine expectation of realising the hope cherished by the Royal Founder of the Imperial Institute, that this memorial of the completion, by our beloved Sovereign, of fifty years of a wise and prosperous reign, is destined to be one of the most important bulwarks of this Country, its Colonies and Dependencies, by becoming a great centre of operations, ceaselessly active in fostering the unity, and developing the resources, and thus maintaining and increasing the power and prosperity, of our Empire.

British Association for the Advancement of Science.

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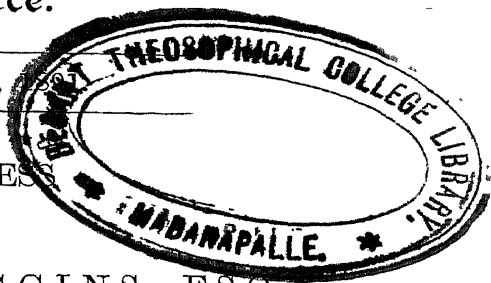
ADDRESS

BY

WILLIAM HUGGINS, ESQ.,

D.C.L. (Oxon.), LL.D. (Cantab., Edin., et Dubl.), PH.D. (Lugd. Bat.),
F.R.S., F.R.A.S., Hon. F.R.S.E., &c., Correspondant
de l'Institut de France,

PRESIDENT.



It is now many years since this Association has done honour to the science of Astronomy in the selection of its President.

Since Sir George Airy occupied the chair in 1851, and the late Lord Wrottesley nine years later in 1860, other sciences have been represented by the distinguished men who have presided over your meetings.

The very remarkable discoveries in our knowledge of the heavens which have taken place during this period of thirty years—one of amazing and ever-increasing activity in all branches of science—have not passed unnoticed in the addresses of your successive Presidents; still it seems to me fitting that I should speak to you to-night chiefly of those newer methods of astronomical research which have led to those discoveries, and which have become possible by the introduction since 1860 into the observatory of the spectroscope and the modern photographic plate.

In 1866 I had the honour of bringing before this Association, at one of the evening lectures, an account of the first-fruits of the novel and unexpected advances in our knowledge of the celestial bodies which followed rapidly upon Kirchhoff's original work on the solar spectrum and the interpretation of its lines.

Since that time a great harvest has been gathered in the same field by many reapers. Spectroscopic astronomy has become a distinct and acknowledged branch of the science, possessing a large literature of its own and observatories specially devoted to it. The more recent discovery of the gelatine dry plate has given a further great impetus to this modern side of astronomy, and has opened a pathway into the unknown of which even an enthusiast thirty years ago would scarcely have dared to dream.

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In no science, perhaps, does the sober statement of the results which have been achieved appeal so strongly to the imagination, and make so evident the almost boundless powers of the mind of man. By means of its light alone to analyse the chemical nature of a far distant body; to be able to reason about its present state in relation to the past and future; to measure within an English mile or less per second the otherwise invisible motion which it may have towards or from us; to do more, to make even that which is darkness to our eyes light, and from vibrations which our organs of sight are powerless to perceive to evolve a revelation in which we see mirrored some of the stages through which the stars may pass in their slow evolutionary progress—surely the record of such achievements, however poor the form of words in which they may be described, is worthy to be regarded as the scientific epic of the present century.

I do not purpose to attempt a survey of the progress of spectroscopic astronomy from its birth at Heidelberg in 1859, but to point out what we do know at present, as distinguished from what we do not know, of a few only of its more important problems, giving a prominent place, in accordance with the traditions of this chair, to the work of the last year or two.

In the spectroscope itself advances have been made by Lord Rayleigh by his discussion of the theory of the instrument, and by Professor Rowland in the construction of concave gratings.

Lord Rayleigh has shown that there is not the necessary connection, sometimes supposed, between dispersion and resolving power, as besides the prism or grating other details of construction and of adjustment of a spectroscope must be taken into account.

The resolving power of the prismatic spectroscope is proportional to the length of path in the dispersive medium. For the heavy flint glass used in Lord Rayleigh's experiments the thickness necessary to resolve the sodium lines came out 1.02 cm. If this be taken as a unit, the resolving power of a prism of similar glass will be in the neighbourhood of the sodium lines equal to the number of centimetres of its thickness. In other parts of the spectrum the resolving power will vary inversely as the third power of the wave-length, so that it will be eight times as great in the violet as in the red. The resolving power of a spectroscope is therefore proportional to the total thickness of the dispersive material in use, irrespective of the number, the angles, or the setting of the separate prisms into which, for the sake of convenience, it may be distributed.

The resolving power of a grating depends upon the total number of lines on its surface, and the order of spectrum in use; about 1,000 lines being necessary to resolve the sodium lines in the first spectrum.

As it is often of importance in the record of observations to state the efficiency of the spectroscope with which they were made, Professor

Schuster has proposed the use of a unit of purity as well as of resolving power, for the full resolving power of a spectroscope is realised in practice only when a sufficiently narrow slit is used. The unit of purity also is to stand for the separation of two lines differing by one-thousandth of their own wave-length; about the separation of the sodium pair at D.

A further limitation may come in from the physiological fact that, as Lord Rayleigh has pointed out, the eye when its full aperture is used is not a perfect instrument. If we wish to realise the full resolving power of a spectroscope, therefore, the emergent beam must not be larger than about one-third of the opening of the pupil.

Up to the present time the standard of reference for nearly all spectroscopic work continues to be Ångström's map of the solar spectrum, and his scale based upon his original determinations of absolute wave-length. It is well known, as was pointed out by Thalén in his work on the spectrum of iron in 1884, that Ångström's figures are slightly too small, in consequence of an error existing in a standard mètre used by him. The corrections for this have been introduced into the tables of the wave-lengths of terrestrial spectra collected and revised by a Committee of this Association from 1885 to 1887. Last year the Committee added a table of corrections to Rowland's scale.

The inconvenience caused by a change of standard scale is, for a time at least, considerable; but there is little doubt that in the near future Rowland's photographic map of the solar spectrum, and his scale based on the determinations of absolute wave-length by Pierce and Bell, or the Potsdam scale based on original determinations by Müller and Kempf, which differs very slightly from it, will come to be exclusively adopted.

The great accuracy of Rowland's photographic map is due chiefly to the introduction by him of concave gratings, and of a method for their use, by which the problem of the determination of relative wave-lengths is simplified to measures of coincidences of the lines in different spectra by a micrometer.

The concave grating and its peculiar mounting, in which no lenses or telescope are needed, and in which all the spectra are in focus together, formed a new departure of great importance in the measurement of spectral lines. The valuable method of photographic sensitizers for different parts of the spectrum has enabled Professor Rowland to include in his map the whole visible solar spectrum, as well as the ultra-violet portion as far as it can get through our atmosphere. Some recent photographs of the solar spectrum, which include A, by Mr. George Higgs, are of great technical beauty.

During the past year the results of three independent researches have appeared, in which the special object of the observers has been to distinguish the lines which are due to our atmosphere from those which are truly solar—the maps of M. Thollon, which, owing to his lamented death

just before their final completion, have assumed the character of a memorial of him; maps by Dr. Becker; and sets of photographs of a high and a low sun by Mr. McClean.

At the meeting of this Association in Bath, M. Janssen gave an account of his own researches on the terrestrial lines of the solar spectrum, which owe their origin to the oxygen of our atmosphere. He discovered the remarkable fact that while one class of bands varies as the density of the gas, other diffuse bands vary as the square of the density. These observations are in accordance with the work of Egoroff and of Olszewski, and of Liveing and Dewar on condensed oxygen. In some recent experiments Olszewski, with a layer of liquid oxygen thirty millimètres thick, saw, as well as four other bands, the band coincident with Fraunhofer's A; a remarkable instance of the persistence of absorption through a great range of temperature. The light which passed through the liquid oxygen had a light blue colour resembling that of the sky.

Of not less interest are the experiments of Knut Ångström, which show that the carbonic acid and aqueous vapour of the atmosphere reveal their presence by dark bands in the invisible infra-red region, at the positions of bands of emission of these substances.

It is now some thirty years since the spectroscope gave us for the first time certain knowledge of the nature of the heavenly bodies, and revealed the fundamental fact that terrestrial matter is not peculiar to the solar system, but is common to all the stars which are visible to us.

In the case of a star such as Capella, which has a spectrum almost identical with that of the sun, we feel justified in concluding that the matter of which it is built up is similar, and that its temperature is also high, and not very different from the solar temperature. The task of analysing the stars and nebulae becomes, however, one of very great difficulty when we have to do with spectra differing from the solar type. We are thrown back upon the laboratory for the information necessary to enable us to interpret the indications of the spectroscope as to the chemical nature, the density and pressure, and the temperature of the celestial masses.

What the spectroscope immediately reveals to us are the waves which were set up in the ether filling all interstellar space, years or hundreds of years ago, by the motions of the molecules of the celestial substances. As a rule it is only when a body is gaseous and sufficiently hot that the motions within its molecules can produce bright lines and a corresponding absorption. The spectra of the heavenly bodies are indeed to a great extent absorption spectra, but we have usually to study them through the corresponding emission spectra of bodies brought into the gaseous form and rendered luminous by means of flames or of electric dis-

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charges. In both cases, unfortunately, as has been shown recently by Professors Liveing and Dewar, Wüllner, E. Wiedemann, and others, there appears to be no certain direct relation between the luminous radiation as shown in the spectroscope and the temperature of the flame, or of the gaseous contents of the vacuum tube, that is, in the usual sense of the term as applied to the mean motion of all the molecules. In both cases, the vibratory motions within the molecules to which their luminosity is due are almost always much greater than would be produced by encounters of molecules having motions of translation no greater than the average motions which characterise the temperature of the gases as a whole. The temperature of a vacuum tube through which an electric discharge is taking place may be low, as shown by a thermometer, quite apart from the consideration of the extreme smallness of the mass of gas, but the vibrations of the luminous molecules must be violent in whatever way we suppose them to be set up by the discharge; if we take Schuster's view that comparatively few molecules are carrying the discharge, and that it is to the fierce encounters of these alone that the luminosity is due, then if all the molecules had similar motions, the temperature of the gas would be very high.

So in flames where chemical changes are in progress, the vibratory motions of the molecules which are luminous may be, in connection with the energy set free in these changes, very different from those corresponding to the mean temperature of the flame.

Under the ordinary conditions of terrestrial experiments, therefore, the temperature or the mean vis viva of the molecules may have no direct relation to the total radiation, which, on the other hand, is the sum of the radiation due to each luminous molecule.

These phenomena have recently been discussed by Ebert from the standpoint of the electro-magnetic theory of light.

Very great caution is therefore called for when we attempt to reason by the aid of laboratory experiments to the temperature of the heavenly bodies from their radiation, especially on the reasonable assumption that in them the luminosity is not ordinarily associated with chemical changes or with electrical discharges, but is due to a simple glowing from the ultimate conversion into molecular motion of the gravitational energy of shrinkage.

In a recent paper Stas maintains that electric spectra are to be regarded as distinct from flame spectra, and from researches of his own, that the pairs of lines of the sodium spectrum other than D are produced only by disruptive electric discharges. As these pairs of lines are found reversed in the solar spectrum, he concludes that the sun's radiation is due mainly to electric discharges. But Wolf and Diacon, and later, Watts, observed the other pairs of lines of the sodium spectrum when the vapour was raised above the ordinary temperature of the Bunsen flame. Recently, Liveing and Dewar saw easily, besides D, the citron and green pairs and

sometimes the blue pair and the orange pair, when hydrogen charged with sodium vapour was burning at different pressures in oxygen. In the case of sodium vapour, therefore, and presumably in all other vapours and gases, it is a matter of indifference whether the necessary vibratory motion of the molecules is produced by electric discharges or by flames. The presence of lines in the solar spectrum which we can only produce electrically is an indication, however, as Stas points out, of the high temperature of the sun.

We must not forget that the light from the heavenly bodies may consist of the combined radiations of different layers of gas at different temperatures, and possibly be further complicated to an unknown extent by the absorption of cooler portions of gas outside.

Not less caution is needed if we endeavour to argue from the broadening of lines and the coming in of a continuous spectrum as to the relative pressure of the gas in the celestial atmospheres. On the one hand, it cannot be gainsaid that in the laboratory the widening of the lines in a Plücker's tube follows upon increasing the density of the residue of hydrogen in the tube, when the vibrations are more frequently disturbed by fresh encounters, and that a broadening of the sodium lines in a flame at ordinary pressure is produced by an increase of the quantity of sodium in the flame; but it is doubtful if pressure, as distinguished from quantity, does produce an increase of the breadth of the lines. An individual molecule of sodium will be sensibly in the same condition, considering the relatively enormous number of the molecules of the other gases, whether the flame is scantily or copiously fed with the sodium salt. With a small quantity of sodium vapour the intensity will be feeble except near the maximum of the lines; when, however, the quantity is increased the comparative transparency on the sides of the maximum will allow the light from the additional molecules met with in the path of the visual ray to strengthen the radiation of the molecules farther back, and so increase the breadth of the lines.

In a gaseous mixture it is found, as a rule, that at the same pressure or temperature, as the encounters with similar molecules become fewer, the spectral lines will be affected as if the body were observed under conditions of reduced quantity or temperature.

In their recent investigation of the spectroscopic behaviour of flames under various pressures up to forty atmospheres, Professors Liveing and Dewar have come to the conclusion that though the prominent feature of the light emitted by flames at high pressure appears to be a strong continuous spectrum, there is not the slightest indication that this continuous spectrum is produced by the broadening of the lines of the same gases at low pressure. On the contrary, photometric observations of the brightness of the continuous spectrum, as the pressure is varied, show that it is mainly produced by the mutual action of the molecules of a gas. Experiments on the sodium spectrum were carried up to a pressure of

forty atmospheres without producing any definite effect on the width of the lines which could be ascribed to the pressure. In a similar way the lines of the spectrum of water showed no signs of expansion up to twelve atmospheres; though more intense than at ordinary pressure, they remained narrow and clearly defined.

It follows, therefore, that a continuous spectrum cannot be considered, when taken alone, as a sure indication of matter in the liquid or the solid state. Not only, as in the experiments already mentioned, such a spectrum may be due to gas when under pressure, but, as Maxwell pointed out, if the thickness of a medium, such as sodium vapour, which radiates and absorbs different kinds of light, be very great, and the temperature high, the light emitted will be of exactly the same composition as that emitted by lamp-black at the same temperature, for the radiations which are feebly emitted will be also feebly absorbed, and can reach the surface from immense depths. Schuster has shown that oxygen, even in a partially exhausted tube, can give a continuous spectrum when excited by a feeble electric discharge.

Compound bodies are usually distinguished by a banded spectrum; but on the other hand such a spectrum does not necessarily show the presence of compounds, that is, of molecules containing different kinds of atoms, but simply of a more complex molecule, which may be made up of similar atoms, and be therefore an allotropic condition of the same body. In some cases, for example, in the diffuse bands of the absorption spectrum of oxygen, the bands may have an intensity proportional to the square of the density of the gas, and may be due either to the formation of more complex molecules of the gas with increase of pressure, or it may be to the constraint to which the molecules are subject during their encounters with one another.

It may be thought that at least in the coincidences of bright lines we are on the solid ground of certainty, since the length of the waves set up in the ether by a molecule, say of hydrogen, is the most fixed and absolutely permanent quantity in nature, and is so of physical necessity, for with any alteration the molecule would cease to be hydrogen.

Such would be the case if the coincidence were certain; but an absolute coincidence can be only a matter of greater or less probability, depending on the resolving power employed, on the number of the lines which correspond and on their characters. When the coincidences are very numerous, as in the case of iron and the solar spectrum, or the lines are characteristically grouped, as in the case of hydrogen and the solar spectrum, we may regard the coincidence as certain; but the progress of science has been greatly retarded by resting important conclusions upon the apparent coincidence of single lines, in spectroscopes of very small resolving power. In such cases, unless other reasons supporting the coincidence are present, the probability of a real coincidence is almost too small to be of any importance, especially in the case of a heavenly

body which may have a motion of approach or of recession of unknown amount.

But even here we are met by the confusion introduced by multiple spectra, corresponding to different molecular groupings of the same substance; and, further, to the influence of substances in vapour upon each other; for when several gases are present together, the phenomena of radiation and reversal by absorption are by no means the same as if the gases were free from each other's influence, and especially is this the case when they are illuminated by an electric discharge.

I have said as much as time will permit, and I think indeed sufficient, to show that it is only by the laborious and slow process of most cautious observation that the foundations of the science of celestial physics can be surely laid. We are at present in a time of transition when the earlier, and, in the nature of things, less precise observations are giving place to work of an order of accuracy much greater than was formerly considered attainable with objects of such small brightness as the stars.

The accuracy of the earlier determinations of the spectra of the terrestrial elements are in most cases insufficient for modern work on the stars as well as on the sun. They fall much below the scale adopted in Rowland's map of the sun, as well as below the degree of accuracy attained at Potsdam by photography in a part of the spectrum for the brighter stars. Increase of resolving power very frequently breaks up into groups, in the spectra of the sun and stars, the lines which had been regarded as single, and their supposed coincidences with terrestrial lines fall to the ground. For this reason many of the early conclusions, based on observation as good as it was possible to make at the time with the less powerful spectroscopes then in use, may not be found to be maintained under the much greater resolving power of modern instruments.

The spectroscope has failed as yet to interpret for us the remarkable spectrum of the Aurora Borealis. Undoubtedly in this phenomenon portions of our atmosphere are lighted up by electric discharges; we should expect, therefore, to recognise the spectra of the gases known to be present in it. As yet we have not been able to obtain similar spectra from these gases artificially, and especially we do not know the origin of the principal line in the green, which often appears alone, and may have therefore an origin independent of that of the other lines. Recently the suggestion has been made that the Aurora is a phenomenon produced by the dust of meteors and falling stars, and that near positions of certain auroral lines to lines or flutings of manganese, lead, barium, thallium, iron, &c., are sufficient to justify us in regarding meteoric dust in the atmosphere as the origin of the auroral spectrum. Liveing and Dewar have made a conclusive research on this point, by availing themselves of the dust of excessive minuteness thrown off from the surface of electrodes of various

metals and meteorites by a disruptive discharge, and carried forward into the tube of observation by a more or less rapid current of air or other gas. These experiments prove that metallic dust, however fine, suspended in a gas will not act like gaseous matter in becoming luminous with its characteristic spectrum in an electric discharge, similar to that of the Aurora. Professor Schuster has suggested that the principal line may be due to some very light gas which is present in too small a proportion to be detected by chemical analysis or even by the spectroscope in the presence of the other gases near the earth, but which at the height of the auroral discharges is in a sufficiently greater relative proportion to give a spectrum. Lemström, indeed, states that he saw this line in the silent discharge of a Holtz machine on a mountain in Lapland. The lines may not have been obtained in our laboratories from the atmospheric gases, on account of the difficulty of reproducing in tubes with sufficient nearness the conditions under which the auroral discharges take place.

In the spectra of comets the spectroscope has shown the presence of carbon presumably in combination with hydrogen, and also sometimes with nitrogen; and in the case of comets approaching very near the sun, the lines of sodium, and other lines which have been supposed to belong to iron. Though the researches of Professor H. A. Newton and of Professor Schiaparelli leave no doubt of the close connection of comets with corresponding periodic meteor swarms, and therefore of the probable identity of cometary matter with that of meteorites, with which the spectroscopic evidence agrees, it would be perhaps unwise at present to attempt to define too precisely the exact condition of the matter which forms the nucleus of the comet. In any case the part of the light of the comet which is not reflected solar light can scarcely be attributed to a high temperature produced by the clashing of separate meteoric stones set up within the nucleus by the sun's disturbing force. We must look rather to disruptive electric discharges produced probably by processes of evaporation due to increased solar heat, which would be amply sufficient to set free portions of the occluded gases into the vacuum of space. May it be that these discharges are assisted, and indeed possibly increased, by the recently discovered action of the ultra-violet part of the sun's light? Lenard and Wolf have shown that ultra-violet light can produce a discharge from a negatively electrified piece of metal, while Hallwachs and Righi have shown further that ultra-violet light can even charge positively an unelectrified piece of metal. Similar actions on cometary matter, unscreened as it is by an absorptive atmosphere, at least of any noticeable extent, may well be powerful when a comet approaches the sun, and help to explain an electrified condition of the evaporated matter which would possibly bring it under the sun's repulsive action. We shall have to return to this point in speaking of the solar corona.

A very great advance has been made in our knowledge of the constitution of the sun by the recent work at the Johns Hopkins University

by means of photography and concave gratings, in comparing the solar spectrum, under great resolving power, directly with the spectra of the terrestrial elements. Professor Rowland has shown that the lines of thirty-six terrestrial elements at least are certainly present in the solar spectrum, while eight others are doubtful. Fifteen elements, including nitrogen as it shows itself under an electric discharge in a vacuum tube, have not been found in the solar spectrum. Some ten other elements, inclusive of oxygen, have not yet been compared with the sun's spectrum.

Rowland remarks that of the fifteen elements named as not found in the sun, many are so classed because they have few strong lines, or none at all, in the limit of the solar spectrum as compared by him with the arc. Boron has only two strong lines. The lines of bismuth are compound and too diffuse. Therefore even in the case of these fifteen elements there is little evidence that they are really absent from the sun.

It follows that if the whole earth were heated to the temperature of the sun, its spectrum would resemble very closely the solar spectrum.

Rowland has not found any lines common to several elements, and in the case of some accidental coincidences, more accurate investigation reveals some slight difference of wave-length or a common impurity. Further, the relative strength of the lines in the solar spectrum is generally, with a few exceptions, the same as that in the electric arc, so that Rowland considers that his experiments show 'very little evidence' of the breaking up of the terrestrial elements in the sun.

Stas in a recent paper gives the final results of eleven years of research on the chemical elements in a state of purity, and on the possibility of decomposing them by the physical and chemical forces at our disposal. His experiments on calcium, strontium, lithium, magnesium, silver, sodium and thallium, show that these substances retain their individuality under all conditions, and are unalterable by any forces that we can bring to bear upon them.

Professor Rowland looks to the solar lines which are unaccounted for as a means of enabling him to discover such new terrestrial elements as still lurk in rare minerals and earths, by confronting their spectra directly with that of the sun. He has already resolved yttrium spectroscopically into three components, and actually into two. The comparison of the results of this independent analytical method with the remarkable but different conclusions to which M. Lecoq de Boisbaudran and Mr. Crookes have been led respectively, from spectroscopic observation of these bodies when glowing under molecular bombardment in a vacuum tube, will be awaited with much interest. It is worthy of remark that as our knowledge of the spectrum of hydrogen in its complete form came to us from the stars, it is now from the sun that chemistry is probably about to be enriched by the discovery of new elements.

In a discussion in the Bakerian lecture for 1885 of what we knew up to that time of the sun's corona, I was led to the conclusion that the

corona is essentially a phenomenon similar in the cause of its formation to the tails of comets, namely, that it consists for the most part probably of matter going from the sun under the action of a force, possibly electrical, which varies as the surface, and can therefore in the case of highly attenuated matter easily master the force of gravity even near the sun. Though many of the coronal particles may return to the sun, those which form the long rays or streamers do not return; they separate and soon become too diffused to be any longer visible, and may well go to furnish the matter of the zodiacal light, which otherwise has not received a satisfactory explanation. And further, if such a force exist at the sun, the changes of terrestrial magnetism may be due to direct electric action, as the earth moves through lines of inductive force

These conclusions appear to be in accordance broadly with the lines along which thought has been directed by the results of subsequent eclipses. Professor Schuster takes an essentially similar view, and suggests that there may be a direct electric connection between the sun and the planets. He asks further whether the sun may not act like a magnet in consequence of its revolution about its axis. Professor Bigelow has recently treated the coronal forms by the theory of spherical harmonics, on the supposition that we see phenomena similar to those of free electricity, the rays being lines of force, and the coronal matter discharged from the sun, or at least arranged or controlled by these forces. At the extremities of the streams for some reasons the repulsive power may be lost, and gravitation set in, bringing the matter back to the sun. The matter which does leave the sun is persistently transported to the equatorial plane of the corona; in fact, the zodiacal light may be the accumulation at great distances from the sun along this equator of such like material. Photographs on a larger scale will be desirable for the full development of the conclusions which may follow from this study of the curved forms of the coronal structure. Professor Schaeberle, however, considers that the coronal phenomena may be satisfactorily accounted for on the supposition that the corona is formed of streams of matter ejected mainly from the spot zones with great initial velocities, but smaller than 382 miles a second. Further that the different types of the corona are due to the effects of perspective on the streams from the earth's place at the time relatively to the plane of the solar equator.

Of the physical and the chemical nature of the coronal matter we know very little. Schuster concludes, from an examination of the eclipses of 1882, 1883, and 1886, that the continuous spectrum of the corona has the maximum of actinic intensity displaced considerably towards the red when compared with the spectrum of the sun, which shows that it can only be due in small part to solar light scattered by small particles. The lines of calcium and of hydrogen do not appear to form part of the normal spectrum of the corona. The green coronal line has no known representative in

terrestrial substances, nor has Schuster been able to recognise any of our elements in the other lines of the corona.

The spectra of the stars are almost infinitely diversified, yet they can be arranged with some exceptions in a series in which the adjacent spectra, especially in the photographic region, are scarcely distinguishable, passing from the bluish-white stars like Sirius, through stars more or less solar in character, to stars with banded spectra, which divide themselves into two apparently independent groups, according as the stronger edge of the bands is towards the red or the blue. In such an arrangement the sun's place is towards the middle of the series.

At present a difference of opinion exists as to the direction in the series in which evolution is proceeding, whether by further condensation white stars pass into the orange and red stages, or whether these more coloured stars are younger and will become white by increasing age. The latter view was suggested by Johnstone Stoney in 1867.

About ten years ago Ritter, in a series of papers, discussed the behaviour of gaseous masses during condensation, and the probable resulting constitution of the heavenly bodies. According to him, a star passes through the orange and red stages twice, first during a comparatively short period of increasing temperature which culminates in the white stage, and a second time during a more prolonged stage of gradual cooling. He suggested that the two groups of banded stars may correspond to these different periods: the young stars being those in which the stronger edge of the dark band is towards the blue, the other banded stars, which are relatively less luminous and few in number, being those which are approaching extinction through age.

Recently a similar evolutional order has been suggested, which is based upon the hypothesis that the nebulae and stars consist of colliding meteoric stones in different stages of condensation.

More recently the view has been put forward that the diversified spectra of the stars do not represent the stages of an evolutional progress, but are due for the most part to differences of original constitution.

The few minutes which can be given to this part of the address are insufficient for a discussion of these different views. I purpose, therefore, to state briefly, and with reserve as the subject is obscure, some of the considerations from the characters of their spectra which appeared to me to be in favour of the evolutional order in which I arranged the stars from their photographic spectra in 1879. This order is essentially the same as Vogel had previously proposed in his classification of the stars in 1874, in which the white stars, which are most numerous, represent the early adult and most persistent stage of stellar life, the solar condition that of full maturity and of commencing age; while in the orange and red stars with banded spectra we see the setting in and advance of old age. But this statement must be taken broadly, and not as asserting that all

stars, however different in mass and possibly to some small extent in original constitution, exhibit one invariable succession of spectra.

In the spectra of the white stars the dark metallic lines are relatively inconspicuous, and occasionally absent, at the same time that the dark lines of hydrogen are usually strong, and more or less broad, upon a continuous spectrum, which is remarkable for its brilliancy at the blue end. In some of these stars the hydrogen and some other lines are bright, and sometimes variable.

As the greater or less prominence of the hydrogen lines, dark or bright, is characteristic of the white stars as a class, and diminishes gradually with the incoming and increase in strength of the other lines, we are probably justified in regarding it as due to some conditions which occur naturally during the progress of stellar life, and not to a peculiarity of original constitution.

To produce a strong absorption-spectrum a substance must be at the particular temperature at which it is notably absorptive; and, further, this temperature must be sufficiently below that of the region behind from which the light comes for the gas to appear, so far as its special rays are concerned, as darkness upon it. Considering the high temperature to which hydrogen must be raised before it can show its characteristic emission and absorption, we shall probably be right in attributing the relative feebleness or absence of the other lines, not to the paucity of the metallic vapours, but rather to their being so hot relatively to the substances behind them as to show feebly, if at all, by reversion. Such a state of things would more probably be found, it seems to me, in conditions anterior to the solar stage. A considerable cooling of the sun would probably give rise to banded spectra due to compounds, or to more complex molecules, which might form near the condensing points of the vapours.

The sun and stars are generally regarded as consisting of glowing vapours surrounded by a photosphere where condensation is taking place, the temperature of the photospheric layer from which the greater part of the radiation comes being constantly renewed from the hotter matter within.

At the surface the convection currents would be strong, producing a considerable commotion, by which the different gases would be mixed and not allowed to retain the inequality of proportions at different levels due to their vapour densities.

Now the conditions of the radiating photosphere and those of the gases above it, on which the character of the spectrum of a star depends, will be determined, not alone by temperature, but also by the force of gravity in these regions; this force will be fixed by the star's mass and its stage of condensation, and will become greater as the star continues to condense.

In the case of the sun the force of gravity has already become so

great at the surface that the decrease of the density of the gases must be extremely rapid passing in the space of a few miles, from atmospheric pressure to a density infinitesimally small; consequently the temperature-gradient at the surface, if determined solely by expansion, must be extremely rapid. The gases here, however, are exposed to the fierce radiation of the sun, and unless wholly transparent would take up heat, especially if any solid or liquid particles were present from condensation or convection currents.

From these causes, within a very small extent of space at the surface of the sun, all bodies with which we are acquainted should fall to a condition in which the extremely tenuous gas could no longer give a visible spectrum. The insignificance of the angle subtended by this space as seen from the earth should cause the boundary of the solar atmosphere to appear defined. If the boundary which we see be that of the sun proper, the matter above it will have to be regarded as in an essentially dynamical condition—an assemblage, so to speak, of gaseous projectiles for the most part falling back upon the sun after a greater or less range of flight. But in any case it is within a space of relatively small extent in the sun and probably in the other solar stars, that the reversion which is manifested by dark lines is to be regarded as taking place.

Passing backward in the star's life, we should find a gradual weakening of gravity at the surface, a reduction of the temperature-gradient so far as it was determined by expansion, and convection currents of less violence producing less interference with the proportional quantities of gases due to their vapour densities, while the effects of eruptions would be more extensive.

At last we might come to a state of things in which, if the star were hot enough, only hydrogen might be sufficiently cool relatively to the radiation behind to produce a strong absorption. The lower vapours would be protected, and might continue to be relatively too hot for their lines to appear very dark upon the continuous spectrum; besides, their lines might be possibly to some extent effaced by the coming in under such conditions in the vapours themselves of a continuous spectrum.

In such a star the light radiated towards the upper part of the atmosphere may have come from portions lower down of the atmosphere itself, or at least from parts not greatly hotter. There may be no such great difference of temperature of the low and less low portions of the star's atmosphere as to make the darkening effect of absorption of the protected metallic vapours to prevail over the illuminating effect of their emission.

It is only by a vibratory motion corresponding to a very high temperature that the bright lines of the first spectrum of hydrogen can be brought out, and by the equivalence of absorbing and emitting power that the corresponding spectrum of absorption should be produced; yet for a strong absorption to show itself, the hydrogen must be cool relatively to the source of radiation behind it, whether this be condensed particles

or gas. Such conditions, it seems to me, should occur in the earlier rather than in the more advanced stages of condensation.

The subject is obscure, and we may go wrong in our mode of conceiving of the probable progress of events, but there can be no doubt that in one remarkable instance the white-star spectrum is associated with an early stage of condensation.

Sirius is one of the most conspicuous examples of one type of this class of stars. Photometric observations combined with its ascertained parallax show that this star emits from forty to sixty times the light of our sun, even to the eye, which is insensible to ultra-violet light, in which Sirius is very rich, while we learn from the motion of its companion that its mass is not much more than double that of our sun. It follows that unless we attribute to this star an improbably great emissive power, it must be of immense size, and in a much more diffuse and therefore an earlier condition than our sun; though probably at a later stage than those white stars in which the hydrogen lines are bright.

A direct determination of the relative temperature of the photospheres of the stars might possibly be obtained in some cases from the relative position of maximum radiation of their continuous spectra. Langley has shown that through the whole range of temperature on which we can experiment, and presumably at temperatures beyond, the maximum of radiation-power in solid bodies gradually shifts upwards in the spectrum from the infra-red through the red and orange, and that in the sun it has reached the blue.

The defined character as a rule of the stellar lines of absorption suggests that the vapours producing them do not at the same time exert any strong power of general absorption. Consequently we should probably not go far wrong, when the photosphere consists of liquid or solid particles, if we could compare select parts of the continuous spectrum between the stronger lines or where they are fewest. It is obvious that if extended portions of different stellar spectra were compared, their true relation would be obscured by the line-absorption.

The increase of temperature, as shown by the rise in the spectrum of the maximum of radiation, may not always be accompanied by a corresponding greater brightness of a star as estimated by the eye, which is an extremely imperfect photometric instrument. Not only is the eye blind to large regions of radiation, but even for the small range of light that we can see the visual effect varies enormously with its colour. According to Professor Langley, the same amount of energy which just enables us to perceive light in the crimson at A would in the green produce a visual effect 100,000 times greater. In the violet the proportional effect would be 1,600, in the blue 62,000, in the yellow 28,000, in the orange 14,000, and in the red 1,200. Captain Abney's recent experiments make the sensitiveness of the eye for the green near F to be 750 times greater than for red about C. It is for this reason, at least in part, that I suggested

in 1864, and have since shown by direct observation, that the spectrum of the nebula in Andromeda, and presumably of similar nebulae, is in appearance only wanting in the red.

The stage at which the maximum radiation is in the green, corresponding to the eye's greatest sensitiveness, would be that in which it could be most favourably measured by eye-photometry. As the maximum rose into the violet and beyond, the star would increase in visual brightness, but not in proportion to the increase of energy radiated by it.

The brightness of a star would be affected by the nature of the substance by which the light was chiefly emitted. In the laboratory solid carbon exhibits the highest emissive power. A stellar stage in which radiation comes, to a large extent, from a photosphere of the solid particles of this substance, would be favourable for great brilliancy. Though the stars are built up of matter essentially similar to that of the sun, it does not follow that the proportion of the different elements is everywhere the same. It may be that the substances condensed in the photospheres of different stars may differ in their emissive powers, but probably not to a great extent.

All the heavenly bodies are seen by us through the tinted medium of our atmosphere. According to Langley, the solar stage of stars is not really yellow, but, even as gauged by our imperfect eyes, would appear bluish-white if we could free ourselves from the deceptive influences of our surroundings.

From these considerations it follows that we can scarcely infer the evolutionary stages of the stars from a simple comparison of their eye-magnitudes. We should expect the white stars to be, as a class, less dense than the stars in the solar stage. As great mass might bring in the solar type of spectrum at a relatively earlier time, some of the brightest of these stars may be very massive and brighter than the sun—for example, the brilliant star Arcturus. For these reasons the solar stars should not only be denser than the white stars, but perhaps, as a class, surpass them in mass and eye-brightness.

It has been shown by Lane that, so long as a condensing gaseous mass remains subject to the laws of a purely gaseous body, its temperature will continue to rise.

The greater or less breadth of the lines of absorption of hydrogen in the white-stars may be due to variations of the depth of the hydrogen in the line of sight, arising from the causes which have been discussed. At the sides of the lines the absorption and emission are feebler than in the middle, and would come out more strongly with a greater thickness of gas.

The diversities among the white stars are nearly as numerous as the individuals of the class. Time does not permit me to do more than to record that in addition to the three sub-classes into which they have been divided by Vogel, Scheiner has recently investigated minor differences as suggested by the character of the third line of hydrogen near G. He

has pointed out too that so far as his observations go the white stars in the constellation of Orion stand alone, with the exception of Algol, in possessing a dark line in the blue which has apparently the same position as a bright line in the great nebula of the same constellation; and Pickering finds in his photographs of the spectra of these stars dark lines corresponding to the principal lines of the bright-line stars, and the planetary nebulae with the exception of the chief nebular line. The association of white stars with nebular matter in Orion, in the Pleiades, in the region of the Milky Way, and in other parts of the heavens, may be regarded as falling in with the view that I have taken.

In the stars possibly further removed from the white class than our sun, belonging to the first division of Vogel's third class, which are distinguished by absorption bands with their stronger edge towards the blue, the hydrogen lines are narrower than in the solar spectrum. In these stars the density-gradient is probably still more rapid, the depth of hydrogen may be less, and possibly the hydrogen molecules may be affected by a larger number of encounters with dissimilar molecules. In some red stars with dark hydrocarbon bands the hydrogen lines have not been certainly observed; if they are really absent, it may be because the temperature has fallen below the point at which hydrogen can exert its characteristic absorption; besides, some hydrogen will have united with the carbon. The coming in of the hydrocarbon bands may indicate a later evolutionary stage, but the temperature may still be high, as acetylene can exist in the electric arc.

A number of small stars more or less similar to those which are known by the names of their discoverers, Wolf and Rayet, have been found by Pickering in his photographs. These are remarkable for several brilliant groups of bright lines, including frequently the hydrogen lines and the line D_3 , upon a continuous spectrum strong in blue and violet rays, in which are also dark lines of absorption. As some of the bright groups appear in his photographs to agree in position with corresponding bright lines in the planetary nebulae, Pickering suggests that these stars should be placed in one class with them, but the brightest nebular line is absent from these stars. The simplest conception of their nature would be that each star is surrounded by a nebula, the bright groups being due to the gaseous matter outside the star. Mr. Roberts, however, has not been able to bring out any indication of nebulosity by prolonged exposure. The remarkable star η Argus may belong to this class of the heavenly bodies.

In the nebulae, the elder Herschel saw portions of the fiery mist or 'shining fluid' out of which the heavens and the earth had been slowly fashioned. For a time this view of the nebulae gave place to that which regarded them as external galaxies, cosmical 'sandheaps,' too remote to be resolved into separate stars; though indeed in 1858 Mr. Herbert

Spencer showed that the observations of nebulæ up to that time were really in favour of an evolutionary progress.

In 1864 I brought the spectroscope to bear upon them; the bright lines which flashed upon the eye showed the source of the light to be glowing gas, and so restored these bodies to what is probably their true place, as an early stage of sidereal life.

At that early time our knowledge of stellar spectra was small. For this reason partly, and probably also under the undue influence of theological opinions then widely prevalent, I unwisely wrote in my original paper in 1864, 'that in these objects we no longer have to do with a special modification of our own type of sun, but find ourselves in presence of objects possessing a distinct and peculiar plan of structure.' Two years later, however, in a lecture before this Association, I took a truer position. 'Our views of the universe,' I said, 'are undergoing important changes; let us wait for more facts with minds unfettered by any dogmatic theory, and therefore free to receive the teaching, whatever it may be, of new observations.'

Let us turn aside for a moment from the nebulæ in the sky to the conclusions to which philosophers had been irresistibly led by a consideration of the features of the solar system. We have before us in the sun and planets obviously not a haphazard aggregation of bodies, but a system resting upon a multitude of relations pointing to a common physical cause. From these considerations Kant and Laplace formulated the nebular hypothesis, resting it on gravitation alone, for at that time the science of the conservation of energy was practically unknown. These philosophers showed how, on the supposition that the space now occupied by the solar system was once filled by a vaporous mass, the formation of the sun and planets could be reasonably accounted for.

By a totally different method of reasoning, modern science traces the solar system backward step by step to a similar state of things at the beginning. According to Helmholtz the sun's heat is maintained by the contraction of his mass, at the rate of about 220 feet a year. Whether at the present time the sun is getting hotter or colder we do not certainly know. We can reason back to the time when the sun was sufficiently expanded to fill the whole space occupied by the solar system, and was reduced to a great glowing nebula. Though man's life, the life of the race perhaps, is too short to give us direct evidence of any distinct stages of so august a process, still the probability is great that the nebular hypothesis, especially in the more precise form given to it by Roche, does represent broadly, notwithstanding some difficulties, the succession of events through which the sun and planets have passed.

The nebular hypothesis of Laplace requires a rotating mass of fluid which at successive epochs became unstable from excess of motion, and left behind rings, or more probably perhaps lumps, of matter from the equatorial regions.

The difficulties to which I have referred have suggested to some thinkers a different view of things, according to which it is not necessary to suppose that one part of the system constitutionally supports another. The whole may consist of a congeries of discrete bodies even if these bodies be the ultimate molecules of matter. The planets may have been formed by the gradual accretion of such discrete bodies. On the view that the material of the condensing solar system consisted of separate particles or masses, we have no longer the fluid pressure which is an essential part of Laplace's theory. Faye, in his theory of evolution from meteorites, has to throw over this fundamental idea of the nebular hypothesis, and he formulates instead a different succession of events in which the outer planets were formed last; a theory which has difficulties of its own.

Professor George Darwin has recently shown, from an investigation of the mechanical conditions of a swarm of meteorites, that on certain assumptions a meteoric swarm might behave as a coarse gas, and in this way bring back the fluid pressure exercised by one part of the system on the other, which is required by Laplace's theory. One chief assumption consists in supposing that such inelastic bodies as meteoric stones might attain the effective elasticity of a high order which is necessary to the theory through the sudden volatilisation of a part of their mass at an encounter, by which what is virtually a violent explosive is introduced between the two colliding stones. Professor Darwin is careful to point out that it must necessarily be obscure as to how a small mass of solid matter can take up a very large amount of energy in a small fraction of a second.

Any direct indications from the heavens themselves, however slight, are of so great value, that I should perhaps in this connection call attention to a recent remarkable photograph by Mr. Roberts of the great nebula in Andromeda. On this plate we seem to have presented to us some stage of cosmical evolution on a gigantic scale. The photograph shows a sort of whirlpool disturbance of the luminous matter which is distributed in a plane inclined to the line of sight, in which a series of rings of bright matter separated by dark spaces, greatly foreshortened by perspective, surround a large undefined central mass. We are ignorant of the parallax of this nebula, but there can be little doubt that we are looking upon a system very remote, and therefore of a magnitude great beyond our power of adequate comprehension. The matter of this nebula, in whatever state it may be, appears to be distributed, as in so many other nebulae, in rings or spiral streams, and to suggest a stage in a succession of evolutionary events not inconsistent with that which the nebular hypothesis requires. To liken this object more directly to any particular stage in the formation of the solar system would be 'to compare things great with small,' and might be indeed to introduce a false analogy; but on the other hand, we should err through an excess of caution if we did

not accept the remarkable features brought to light by this photograph as a presumptive indication of a progress of events in cosmical history following broadly upon the lines of Laplace's theory.

The old view of the original matter of the nebulæ, that it consisted of a 'fiery mist,'

'a tumultuous cloud
Instinct with fire and nitre,'

fell at once with the rise of the science of thermodynamics. In 1854 Helmholtz showed that the supposition of an original fiery condition of the nebulous stuff was unnecessary, since in the mutual gravitation of widely separated matter we have a store of potential energy sufficient to generate the high temperature of the sun and stars. We can scarcely go wrong in attributing the light of the nebulæ to the conversion of the gravitational energy of shrinkage into molecular motion.

The idea that the light of comets and of nebulæ may be due to a succession of ignited flashes of gas from the encounters of meteoric stones was suggested by Professor Tait, and was brought to the notice of this Association in 1871 by Sir William Thomson in his Presidential Address.

The spectrum of the bright-line nebulæ is certainly not such a spectrum as we should expect from the flashing by collisions of meteorites similar to those which have been analysed in our laboratories. The strongest lines of the substances which in the case of such meteorites would first show themselves, iron, sodium, magnesium, nickel, &c., are not those which distinguish the nebular spectrum. On the contrary, this spectrum is chiefly remarkable for a few brilliant lines, very narrow and defined, upon a background of a faint continuous spectrum, which contains numerous bright lines, and probably some lines of absorption.

The two most conspicuous lines have not been interpreted; for though the second line falls near, it is not coincident with a strong double line of iron. It is hardly necessary to say that though the near position of the brightest line to the bright double line of nitrogen, as seen in a small spectroscope in 1864, naturally suggested at that early time the possibility of the presence of this element in the nebulæ, I have been careful to point out, to prevent misapprehension, that in more recent years the nitrogen line and subsequently a lead line have been employed by me solely as fiducial points of reference in the spectrum.

The third line we know to be the second line of the first spectrum of hydrogen. Mr. Keeler has seen the first hydrogen line in the red, and photographs show that this hydrogen spectrum is probably present in its complete form, or nearly so, as we first learnt to know it in the absorption spectrum of the white stars.

We are not surprised to find associated with it the line D_3 , near the position of the absent sodium lines, probably due to the atom of some unknown gas, which in the sun can only show itself in the outbursts of

highest temperature, and for this reason does not reveal itself by absorption in the solar spectrum.

It is not unreasonable to assume that the two brightest lines, which are of the same order, are produced by substances of a similar nature, in which a vibratory motion corresponding to a very high temperature is also necessary. These substances, as well as that represented by the line D_3 , may be possibly some of the unknown elements which are wanting in our terrestrial chemistry between hydrogen and lithium, unless indeed D_3 be on the lighter side of hydrogen.

In the laboratory we must have recourse to the electric discharge to bring out the spectrum of hydrogen; but in a vacuum-tube, though the radiation may be great, from the relative fewness of the luminous atoms or molecules or from some other cause, the temperature of the gas as a whole may be low.

On account of the large extent of the nebulæ, a comparatively small number of luminous molecules or atoms would probably be sufficient to make the nebulæ as bright as they appear to us. On such an assumption the average temperature may be low, but the individual particles, which by their encounters are luminous, must have motions corresponding to a very high temperature, and in this sense be extremely hot.

In such diffuse masses, from the great mean length of free path, the encounters would be rare but correspondingly violent, and tend to bring about vibrations of comparatively short period, as appears to be the case if we may judge by the great relative brightness of the more refrangible lines of the nebular spectrum.

Such a view may perhaps reconcile the high temperature which the nebular spectrum undoubtedly suggests with the much lower mean temperature of the gaseous mass, which we should expect at so early a stage of condensation, unless we assume a very enormous mass; or that the matter coming together had previously considerable motion, or considerable molecular agitation.

The inquisitiveness of the human mind does not allow us to remain content with the interpretation of the present state of the cosmical masses, but suggests the question—

‘What see’st thou else

In the dark backward and abysm of time?’

What was the original state of things? how has it come about that by the side of ageing worlds we have nebulæ in a relatively younger stage? Have any of them received their birth from dark suns, which have collied into new life, and so belong to a second or later generation of the heavenly bodies?

During the short historic period, indeed, there is no record of such an event; still it would seem to be only through the collision of dark suns, of which the number must be increasing, that a temporary rejuvenescence

of the heavens is possible, and by such ebbings and flowings of stellar life that the inevitable end to which evolution in its apparently uncompensated progress is carrying us can, even for a little, be delayed.

We cannot refuse to admit as possible such an origin for nebulæ.

In considering, however, the formation of the existing nebulæ we must bear in mind that, in the part of the heavens within our ken, the stars still in the early and middle stages of evolution exceed greatly in number those which appear to be in an advanced condition of condensation. Indeed, we find some stars which may be regarded as not far advanced beyond the nebular condition.

It may be that the cosmical bodies which are still nebulous owe their later development to some conditions of the part of space where they occur, such as conceivably a greater original homogeneity, in consequence of which condensation began less early. In other parts of space condensation may have been still further delayed, or even have not yet begun. It is worthy of remark that these nebulæ group themselves about the Milky Way, where we find a preponderance of the white-star type of stars, and almost exclusively the bright-line stars which Pickering associates with the planetary nebulæ. Further, Dr. Gill concludes, from the rapidity with which they impress themselves upon the plate, that the fainter stars of the Milky Way also, to a large extent, belong to this early type of stars. At the same time other types of stars occur also over this region, and the red hydrocarbon stars are found in certain parts; but possibly these stars may be before or behind the Milky Way, and not physically connected with it.

If light matter be suggested by the spectrum of these nebulæ, it may be asked further, as a pure speculation, whether in them we are witnessing possibly a later condensation of the light matter which had been left behind, at least in a relatively greater proportion, after the first growth of worlds into which the heavier matter condensed, though not without some entanglement of the lighter substances. The wide extent and great diffuseness of this bright-line nebulosity over a large part of the constellation of Orion may be regarded perhaps as pointing in this direction. The diffuse nebulous matter streaming round the Pleiades may possibly be another instance, though the character of its spectrum has not yet been ascertained.

In the planetary nebulæ, as a rule, there is a sensible increase of the faint continuous spectrum, as well as a slight thickening of the bright lines towards the centre of the nebula, appearances which are in favour of the view that these bodies are condensing gaseous masses.

Professor G. Darwin, in his investigation of the equilibrium of a rotating mass of fluid, found, in accordance with the independent researches of Poincaré, that when a portion of the central body becomes detached through increasing angular velocity, the portion should bear a far larger ratio to the remainder than is observed in the planets and satellites of the

solar system, even taking into account heterogeneity from the condensation of the parent mass.

Now this state of things, in which the masses though not equal are of the same order, does seem to prevail in many nebulae, and to have given birth to a large class of binary stars. Mr. See has recently investigated the evolution of bodies of this class, and points out their radical differences from the solar system in the relatively large mass-ratios of the component bodies, as well as in the high eccentricities of their orbits brought about by tidal friction, which would play a more important part in the evolution of such systems.

Considering the large number of these bodies, he suggests that the solar system should perhaps no longer be regarded as representing celestial evolution in its normal form—

‘A goodly Paterne to whose perfect mould
He fashioned them . . .’

but rather as modified by conditions which are exceptional.

It may well be that in the very early stages condensing masses are subject to very different conditions, and that condensation may not always begin at one or two centres, but sometimes set in at a large number of points, and proceed in the different cases along very different lines of evolution.

Besides its more direct use in the chemical analysis of the heavenly bodies, the spectroscope has given to us a great and unexpected power of advance along the lines of the older astronomy. In the future a higher value may, indeed, be placed upon this indirect use of the spectroscope than upon its chemical revelations.

By no direct astronomical methods could motions of approach or of recession of the stars be even detected, much less could they be measured. A body coming directly towards us or going directly from us appears to stand still. In the case of the stars we can receive no assistance from change of size or of brightness. The stars show no true discs in our instruments, and the nearest of them is so far off that if it were approaching us at the rate of a hundred miles in a second of time, a whole century of such rapid approach would not do more than increase its brightness by the one-fortieth part.

Still it was only too clear that, so long as we were unable to ascertain directly those components of the stars' motions which lie in the line of sight, the speed and direction of the solar motion in space, and many of the great problems of the constitution of the heavens, must remain more or less imperfectly known. Now the spectroscope has placed in our hands this power, which, though so essential, appeared almost in the nature of things to lie for ever beyond our grasp; it enables us to measure directly, and under favourable circumstances to within a mile per second,

or even less, the speed of approach or of recession of a heavenly body. This method of observation has the great advantage for the astronomer of being independent of the distance of the moving body, and is therefore as applicable and as certain in the case of a body on the extreme confines of the visible universe, so long as it is bright enough, as in the case of a neighbouring planet.

Doppler had suggested as far back as 1841 that the same principle, on which he had shown that a sound should become sharper or flatter if there were an approach or a recession between the ear and the source of the sound, would apply equally to light; and he went on to say that the difference of colour of some of the binary stars might be produced in this way by their motions. Doppler was right in that the principle is true in the case of light, but he was wrong in the particular conclusion which he drew from it. Even if we suppose a star to be moving with a sufficiently enormous velocity to alter sensibly its colour to the eye, no such change would actually be seen, for the reason that the store of invisible light beyond both limits of the visible spectrum, the blue and the red, would be drawn upon, and light-waves invisible to us would be exalted or degraded so as to take the place of those raised or lowered in the visible region, and the colour of the star would remain unchanged. About eight years later Fizeau pointed out the importance of considering the individual wave-lengths of which white light is composed. As soon, however, as we had learned to recognise the lines of known substances in the spectra of the heavenly bodies, Doppler's principle became applicable as the basis of a new and most fruitful method of investigation. The measurement of the small shift of the celestial lines from their true positions, as shown by the same lines in the spectrum of a terrestrial substance, gives to us the means of ascertaining directly in miles per second the speed of approach or of recession of the heavenly body from which the light has come.

An account of the first application of this method of research to the stars, which was made in my observatory in 1868, was given by Sir Gabriel Stokes from this chair at the meeting at Exeter in 1869. The stellar motions determined by me were shortly after confirmed by Professor Vogel in the case of Sirius, and in case of other stars by Mr. Christie, now Astronomer Royal, at Greenwich; but, necessarily, in consequence of the inadequacy of the instruments then in use for so delicate an inquiry, the amounts of these motions were but approximate.

The method was shortly afterwards taken up systematically at Greenwich and at the Rugby Observatory. It is to be greatly regretted that, for some reasons, the results have not been sufficiently accordant and accurate for a research of such exceptional delicacy. On this account probably, as well as that the spectroscope at that early time had scarcely become a familiar instrument in the observatory, astronomers were slow in availing themselves of this new and remarkable power of investigation.

That this comparative neglect of so truly wonderful a method of ascertaining what was otherwise outside our powers of observation has greatly retarded the progress of astronomy during the last fifteen years, is but too clearly shown by the brilliant results which within the last couple of years have followed fast upon the recent masterly application of this method by photography at Potsdam, and by eye with the needful accuracy at the Lick Observatory. At last this use of the spectroscope has taken its true place as one of the most potent methods of astronomical research. It gives us the motions of approach and of recession, not in angular measures, which depend for their translation into actual velocities upon separate determinations of parallactic displacements, but at once in terrestrial units of distance.

This method of work will doubtless be very prominent in the astronomy of the near future, and to it probably we shall have to look for the more important discoveries in sidereal astronomy which will be made during the coming century.

In his recent application of photography to this method of determining celestial motions, Professor Vogel, assisted by Dr. Scheiner, considering the importance of obtaining the spectrum of as many stars as possible on an extended scale without an exposure inconveniently long, wisely determined to limit the part of the spectrum on the plate to the region for which the ordinary silver-bromide gelatine plates are most sensitive, namely, to a small distance on each side of G, and to employ as the line of comparison the hydrogen line near G, and recently also certain lines of iron. The most minute and complete mechanical arrangements were provided for the purpose of securing the absolute rigidity of the comparison spectrum relatively to that of the star, and for permitting temperature adjustments and other necessary ones to be made.

The perfection of these spectra is shown by the large number of lines, no fewer than 250 in the case of Capella, within the small region of the spectrum on the plate. Already the motions of about fifty stars have been measured with an accuracy, in the case of the larger number of them, of about an English mile per second.

At the Lick Observatory it has been shown that observations can be made directly by eye with an accuracy equally great. Mr. Keeler's brilliant success has followed in great measure from the use of the third and fourth spectra of a grating 14,438 lines to the inch. The marvellous accuracy attainable in his hands on a suitable star is shown by observations on three nights of the star Arcturus, the largest divergence of his measures being not greater than six-tenths of a mile per second, while the mean of the three nights' work agreed with the mean of five photographic determinations of the same star at Potsdam to within one-tenth of an English mile. These are determinations of the motions of a sun so stupendously remote that even the method of parallax practically fails to fathom the depth of intervening space, and by means of light-

waves which have been, according to Elkin's nominal parallax, nearly 200 years upon their journey.

Mr. Keeler with his magnificent means has accomplished a task which I attempted in vain in 1874, with the comparatively poor appliances at my disposal, of measuring the motions in the line of sight of some of the planetary nebulae. As the stars have considerable motions in space it was to be expected that nebulae should possess similar motions, for the stellar motions must have belonged to the nebulae out of which they have been evolved. My instrumental means, limiting my power of detection to motions greater than twenty-five miles per second, were insufficient. Mr. Keeler has found in the examination of ten nebulae motions varying from two miles to twenty-seven miles, with one exceptional motion of nearly forty miles.

For the nebula of Orion, Mr. Keeler finds a motion of recession of about ten miles a second. Now this motion agrees closely with what it should appear to have from the drift of the solar system itself, so far as it has been possible at present to ascertain the probable velocity of the sun in space. This grand nebula, of vast extent and of extreme tenuity, is probably more nearly at rest relatively to the stars of our system than any other celestial object we know; still it would seem more likely that even here we have some motion, small though it may be, than that the motions of the matter of which it is formed were so absolutely balanced as to leave this nebula in the unique position of absolute immobility in the midst of whirling and drifting suns and systems of suns.

The spectroscopic method of determining celestial motions in the line of sight has recently become fruitful in a new but not altogether unforeseen direction, for it has, so to speak, given us a separating power far beyond that of any telescope the glass-maker and the optician could construct, and so enabled us to penetrate into mysteries hidden in stars apparently single, and altogether unsuspected of being binary systems. The spectroscope has not simply added to the list of the known binary stars, but has given to us for the first time a knowledge of a new class of stellar systems, in which the components are in some cases of nearly equal magnitude, and in close proximity, and are revolving with velocities greatly exceeding the planetary velocities of our system.

The K line in the photographs of Mizar, taken at the Harvard College Observatory, was found to be double at intervals of fifty-two days. The spectrum was therefore not due to a single source of light, but to the combined effect of two stars moving periodically in opposite directions in the line of sight. It is obvious that if two stars revolve round their common centre of gravity in a plane not perpendicular to the line of sight, all the lines in a spectrum common to the two stars will appear alternately single or double.

In the case of Mizar and the other stars to be mentioned, the spec-

troscopic observations are not as yet extended enough to furnish more than an approximate determination of the elements of their orbits.

Mizar especially, on account of its relatively long period, about 105 days, needs further observations. The two stars are moving each with a velocity of about fifty miles a second, probably in elliptical orbits, and are about 143 millions of miles apart. The stars of about equal brightness have together a mass about forty times as great as that of our sun.

A similar doubling of the lines showed itself in the Harvard photographs of β Aurigæ at the remarkably close interval of almost exactly two days, indicating a period of revolution of about four days. According to Vogel's later observations, each star has a velocity of nearly seventy miles a second, the distance between the stars being little more than seven and a half millions of miles, and the mass of the system 4.7 times that of the sun. The system is approaching us at the speed of about sixteen miles a second.

The telescope could never have revealed to us double stars of this order. In the case of β Aurigæ, combining Vogel's distance with Pritchard's recent determination of the star's parallax, the greatest angular separation of the stars as seen from the earth would be 1-200th part of a second of arc, and therefore very far too small for the highest powers of the largest telescopes. If we take the relation of aperture to separating power usually accepted, an object glass of about eighty feet in diameter would be needed to resolve this binary star. The spectroscope, which takes no note of distance, magnifies, so to speak, this minute angular separation 4,000 times; in other words, the doubling of the lines, which is the phenomenon that we have to observe, amounts to the easily measurable quantity of twenty seconds of arc.

There were known, indeed, variable stars of short period, which it had been suggested might be explained on the hypothesis of a dark body revolving about a bright sun in a few days, but this theory was met by the objection that no such systems of closely revolving suns were known to exist.

The Harvard photographs of which we have been speaking were taken with a slitless form of spectroscope, the prisms being placed, as originally by Fraunhofer, before the object glass of the telescope. This method, though it possesses some advantages, has the serious drawback of not permitting a direct comparison of the star's spectrum with terrestrial spectra. It is obviously unsuited to a variable star like Algol, where one star only is bright, for in such a case there would be no doubling of the lines, but only a small shift to and fro of the lines of the bright star as it moved in its orbit alternately towards and from our system, which would need for its detection the fiducial positions of terrestrial lines compared directly with them.

For such observations the Potsdam spectrograph was well adapted. Professor Vogel found that the bright star of Algol did pulsate back-

wards and forwards in the visual direction in a period corresponding to the known variation of its light. The explanation which had been suggested for the star's variability, that it was partially eclipsed at regular intervals of 68·8 hours by a dark companion large enough to cut off nearly five-sixths of its light, was therefore the true one. The dark companion, no longer able to hide itself by its obscureness, was brought out into the light of direct observation by means of its gravitational effects.

Seventeen hours before minimum Algol is receding at the rate of about $24\frac{1}{2}$ miles a second, while seventeen hours after minimum it is found to be approaching with a speed of about $28\frac{1}{2}$ miles. From these data, together with those of the variation of its light, Vogel found, on the assumption that both stars have the same density, that the companion, nearly as large as the sun, but with about one-fourth his mass, revolves with a velocity of about fifty-five miles a second. The bright star of about twice the size and mass moves about the common centre of gravity with the speed of about twenty-six miles a second. The system of the two stars, which are about $3\frac{1}{4}$ millions of miles apart, considered as a whole, is approaching us with a velocity of 2·4 miles a second. The great difference in luminosity of the two stars, not less than fifty times, suggests rather that they are in different stages of condensation, and dissimilar in density.

It is obvious that if the orbit of a star with an obscure companion is inclined to the line of sight, the companion will pass above or below the bright star and produce no variation of its light. Such systems may be numerous in the heavens. In Vogel's photographs, Spica, which is not variable, by a small shifting of its lines reveals a backward and forward periodical pulsation due to orbital motion. As the pair whirl round their common centre of gravity, the bright star is sometimes advancing, at others receding. They revolve in about four days, each star moving with a velocity of about fifty-six miles a second in an orbit probably nearly circular, and possess a combined mass of rather more than $2\frac{1}{2}$ times that of the sun. Taking the most probable value for the star's parallax, the greatest angular separation of the stars would be far too small to be detected with the most powerful telescopes.

If in a close double star the fainter companion is of the white-star type, while the bright star is solar in character, the composite spectrum would be solar with the hydrogen lines unusually strong. Such a spectrum would in itself afford some probability of a double origin, and suggest the existence of a companion star.

In the case of a true binary star the orbital motions of the pair would reveal themselves in a small periodical swaying of the hydrogen lines relatively to the solar ones.

Professor Pickering considers that his photographs show ten stars with composite spectra; of these, five are known to be double. The

others are : τ Persei, ζ Aurigæ, δ Sagittarii, 31 Ceti, and β Capricorni. Perhaps β Lyræ should be added to this list.

In his recent classical work on the rotation of the sun, Dunér has not only determined the solar rotation for the equator but for different parallels of latitude up to 75° . The close accordance of his results shows that these observations are sufficiently accurate to be discussed with the variation of the solar rotation for different latitudes, which had been determined by the older astronomical methods from the observations of the solar spots.

Though I have already spoken incidentally of the invaluable aid which is furnished by photography in some of the applications of the spectroscope to the heavenly bodies, the new power which modern photography has put into the hands of the astronomer is so great, and has led already, within the last few years, to new acquisitions of knowledge of such vast importance, that it is fitting that a few sentences should be specially devoted to this subject.

Photography is no new discovery, being about half a century old ; it may excite surprise, and indeed possibly suggest some apathy on the part of astronomers, that though the suggestion of the application of photography to the heavenly bodies dates from the memorable occasion when, in 1839, Arago, announcing to the Académie des Sciences the great discovery of Niepce and Daguerre, spoke of the possibility of taking pictures of the sun and moon by the new process, yet that it is only within a few years that notable advances in astronomical methods and discovery have been made by its aid.

The explanation is to be found in the comparative unsuitability of the earlier photographic methods for use in the observatory. In justice to the early workers in astronomical photography, among whom Bond, De la Rue, J. W. Draper, Rutherford, Gould, hold a foremost place, it is needful to state clearly that the recent great successes in astronomical photography are not due to greater skill, nor, to any great extent, to superior instruments, but to the very great advantages which the modern gelatine dry plate possesses for use in the observatory over the methods of Daguerre, and even over the wet collodion film on glass which, though a great advance on the silver plate, went but a little way towards putting into the hands of the astronomer a photographic surface adapted fully to his wants.

The modern silver-bromide gelatine plate, except for its grained texture, meets the needs of the astronomer at all points. It possesses extreme sensitiveness ; it is always ready for use ; it can be placed in any position ; it can be exposed for hours ; lastly, it does not need immediate development, and for this reason can be exposed again to the same object on succeeding nights, so as to make up by several instalments, as the weather may permit, the total time of exposure which is deemed necessary.

Without the assistance of photography, however greatly the resources of genius might overcome the optical and mechanical difficulties of constructing large telescopes, the astronomer would have to depend in the last resource upon his eye. Now we cannot by the force of continued looking bring into view an object too feebly luminous to be seen at the first and keenest moment of vision. But the feeblest light which falls upon the plate is not lost, but is taken in and stored up continuously. Each hour the plate gathers up 3,600 times the light-energy which it received during the first second. It is by this power of accumulation that the photographic plate may be said to increase, almost without limit, though not in separating power, the optical means at the disposal of the astronomer for the discovery or the observation of faint objects.

Two principal directions may be pointed out in which photography is of great service to the astronomer. It enables him within the comparatively short time of a single exposure to secure permanently with great exactness the relative positions of hundreds or even of thousands of stars, or the minute features of nebulae or other objects, or the phenomena of a passing eclipse, a task which by means of the eye and hand could only be accomplished, if done at all, after a very great expenditure of time and labour. Photography puts it in the power of the astronomer to accomplish in the short span of his own life, and so enter into their fruition, great works which otherwise must have been passed on by him as an heritage of labour to succeeding generations.

The second great service which photography renders is not simply an aid to the powers the astronomer already possesses. On the contrary, the plate, by recording light-waves which are both too small and too large to excite vision in the eye, brings him into a new region of knowledge, such as the infra-red and the ultra-violet parts of the spectrum, which must have remained for ever unknown but for artificial help.

The present year will be memorable in astronomical history for the practical beginning of the Photographic Chart and Catalogue of the Heavens, which took their origin in an International Conference which met in Paris in 1887, by the invitation of M. l'Amiral Mouchez, Director of the Paris Observatory.

The richness in stars down to the ninth magnitude of the photographs of the comet of 1882 taken at the Cape Observatory under the superintendence of Dr. Gill, and the remarkable star charts of the Brothers Henry which followed two years later, astonished the astronomical world. The great excellence of these photographs, which was due mainly to the superiority of the gelatine plate, suggested to these astronomers a complete map of the sky, and a little later gave birth in the minds of the Paris astronomers to the grand enterprise of an International Chart of the Heavens. The actual beginning of the work this year is in no small degree due to the great energy and tact with which the Director of the Paris Observatory has conducted the initial steps, through the many

delicate and difficult questions which have unavoidably presented themselves in an undertaking which depends upon the harmonious working in common of many nationalities, and of no fewer than eighteen observatories in all parts of the world. The three years since 1887 have not been too long for the detailed organisation of this work, which has called for several elaborate preliminary investigations on special points in which our knowledge was insufficient, and which have been ably carried out by Professors Vogel and Bakhuyzen, Dr. Trépied, Dr. Scheiner, Dr. Gill, the Astronomer Royal, and others. Time also was required for the construction of the new and special instruments.

The decisions of the Conference in their final form provide for the construction of a great photographic chart of the heavens with exposures corresponding to forty minutes' exposure at Paris, which it is expected will reach down to stars of about the fourteenth magnitude. As each plate is to be limited to four square degrees, and as each star, to avoid possible errors, is to appear on two plates, over 22,000 photographs will be required. For the more accurate determination of the positions of the stars, a *réseau* with lines at distances of 5 mm. apart is to be previously impressed by a faint light upon the plate, so that the image of the *réseau* will appear together with the images of the stars when the plate is developed. This great work will be divided, according to their latitudes, among eighteen observatories provided with similar instruments, though not necessarily constructed by the same maker. Those in the British dominions and at Tacubaya have been constructed by Sir Howard Grubb.

Besides the plates to form the great chart, a second set of plates for a catalogue is to be taken, with a shorter exposure, which will give stars to the eleventh magnitude only. These plates, by a recent decision of the Permanent Committee, are to be pushed on as actively as possible, though as far as may be practicable plates for the chart are to be taken concurrently. Photographing the plates for the catalogue is but the first step in this work, and only supplies the data for the elaborate measurements which have to be made, which are, however, less laborious than would be required for a similar catalogue without the aid of photography.

Already Dr. Gill has nearly brought to conclusion, with the assistance of Professor Kapteyn, a preliminary photographic survey of the Southern heavens.

With an exposure sufficiently long for the faintest stars to impress themselves upon the plate, the accumulating action still goes on for the brighter stars, producing a great enlargement of their images from optical and photographic causes. The question has occupied the attention of many astronomers whether it is possible to find a law connecting the diameters of these more or less over-exposed images with the relative brightness of the stars themselves. The answer will come out undoubtedly in the affirmative, though at present the empirical formulæ which

have been suggested for this purpose differ from each other. Captain Abney proposes to measure the total photographic action, including density as well as size, by the obstruction which the stellar image offers to light.

A further question follows as to the relation which the photographic magnitudes of stars bear to those determined by eye. Visual magnitudes are the physiological expression of the eye's integration of that part of the star's light which extends from the red to the blue. Photographic magnitudes represent the plate's integration of another part of the star's light, namely, from a little below where the power of the eye leaves off in the blue, to where the light is cut off by the glass, or is greatly reduced by want of proper corrections when a refracting telescope is used. It is obvious that the two records are taken by different methods in dissimilar units of different parts of the star's light. In the case of certain coloured stars the photographic brightness is very different from the visual brightness; but in all stars changes, especially of a temporary character, may occur in the photographic or the visual region, unaccompanied by a similar change in the other part of the spectrum. For these reasons it would seem desirable that the two sets of magnitudes should be tabulated independently, and be regarded as supplementary of each other.

The determination of the distances of the fixed stars from the small apparent shift of their positions when viewed from widely separated positions of the earth in its orbit is one of the most refined operations of the observatory. The great precision with which this minute angular quantity, a fraction of a second only, has to be measured, is so delicate an operation with the ordinary micrometer, though, indeed, it was with this instrument that the classical observations of Sir Robert Ball were made, that a special instrument, in which the measures are made by moving the two halves of a divided object glass, known as a heliometer, has been pressed into this service, and quite recently, in the skilful hands of Dr. Gill and Dr. Elkin, has largely increased our knowledge in this direction.

It is obvious that photography might be here of great service, if we could rely upon measurements of photographs of the same stars taken at suitable intervals of time. Professor Pritchard, to whom is due the honour of having opened this new path, aided by his assistants, has proved by elaborate investigations that measures for parallax may be safely made upon photographic plates, with, of course, the advantages of leisure and repetition; and he has already by this method determined the parallax for twenty-one stars with an accuracy not inferior to that of values previously obtained by purely astronomical methods.

The remarkable successes of astronomical photography, which depend upon the plate's power of accumulation of a very feeble light acting continuously through an exposure of several hours, are worthy to be regarded as a new revelation. The first chapter opened when, in 1880, Dr

Henry Draper obtained a picture of the nebula of Orion ; but a more important advance was made in 1883, when Dr. Common, by his photographs, brought to our knowledge details and extensions of this nebula hitherto unknown. A further disclosure took place in 1885, when the Brothers Henry showed for the first time in great detail the spiral nebulosity issuing from the bright star Maia of the Pleiades, and shortly afterwards nebulous streams about the other stars of this group. In 1886 Mr. Roberts, by means of a photograph to which three hours' exposure had been given, showed the whole background of this group to be nebulous. In the following year Mr. Roberts more than doubled for us the great extension of the nebular region which surrounds the trapezium in the constellation of Orion. By his photographs of the great nebula in Andromeda, he has shown the true significance of the dark canals which had been seen by the eye. They are in reality spaces between successive rings of bright matter, which appeared nearly straight owing to the inclination in which they lie relatively to us. These bright rings surround an undefined central luminous mass. I have already spoken of this photograph.

Some recent photographs by Mr. Russell show that the great rift in the Milky Way in Argus, which to the eye is void of stars, is in reality uniformly covered with them. Also quite recently Mr. George Hale has photographed the prominences by means of a grating, making use of the lines H and K.

The heavens are richly but very irregularly inwrought with stars. The brighter stars cluster into well-known groups upon a background formed of an enlacement of streams and convoluted windings and intertwined spirals of fainter stars, which becomes richer and more intricate in the irregularly rifted zone of the Milky Way.

We, who form part of the emblazonry, can only see the design distorted and confused ; here crowded, there scattered, at another place superposed. The groupings due to our position are mixed up with those which are real.

Can we suppose that each luminous point has no relation to the others near it than the accidental neighbourhood of grains of sand upon the shore, or of particles of the wind-blown dust of the desert ? Surely every star from Sirius and Vega down to each grain of the light-dust of the Milky Way has its present place in the heavenly pattern from the slow evolving of its past. We see a system of systems, for the broad features of clusters and streams and spiral windings which mark the general design are reproduced in every part. The whole is in motion, each point shifting its position by miles every second, though from the august magnitude of their distances from us and from each other, it is only by the accumulated movements of years or of generations that some small changes of relative position reveal themselves.

The deciphering of this wonderfully intricate constitution of the heavens will be undoubtedly one of the chief astronomical works of the coming century. The primary task of the sun's motion in space together with the motions of the brighter stars has been already put well within our reach by the spectroscopic method of the measurement of star-motions in the line of sight.

From other directions information is accumulating: from photographs of clusters and parts of the Milky Way, by Roberts in this country, Barnard at the Lick Observatory, and Russell at Sydney; from the counting of stars, and the detection of their configurations, by Holden and by Backhouse; from the mapping of the Milky Way by eye, at Parsonstown; from photographs of the spectra of stars, by Pickering at Harvard and in Peru; and from the exact portraiture of the heavens in the great international star chart which begins this year.

I have but touched some only of the problems of the newer side of astronomy. There are many others which would claim our attention if time permitted. The researches of the Earl of Rosse on lunar radiation, and the work on the same subject and on the sun, by Langley. Observations of lunar heat with an instrument of his own invention by Mr. Boys; and observations of the variation of the moon's heat with its phase by Mr. Frank Very. The discovery of the ultra-violet part of the hydrogen spectrum, not in the laboratory, but from the stars. The confirmation of this spectrum by terrestrial hydrogen in part by H. W. Vogel, and in its all but complete form by Cornu, who found similar series in the ultra-violet spectra of aluminium and thallium. The discovery of a simple formula for the hydrogen series by Balmer. The important question as to the numerical spectral relationship of different substances, especially in connection with their chemical properties; and the further question as to the origin of the harmonic and other relations between the lines and the groupings of lines of spectra; on these points contributions during the past year have been made by Rudolf v. Kövesligethy, Ames, Hartley, Deslandres, Rydberg, Grünwald, Kayser and Runge, Johnstone Stoney, and others. The remarkable employment of interference phenomena by Professor Michelson for the determination of the size, and distribution of light within them, of the images of objects which when viewed in a telescope subtend an angle less than that subtended by the light-wave at a distance equal to the diameter of the objective. A method applicable not alone to celestial objects, but also to spectral lines, and other questions of molecular physics.

Along the older lines there has not been less activity; by newer methods, by the aid of larger or more accurately constructed instruments, by greater refinement of analysis, knowledge has been increased, especially in precision and minute exactness.

Astronomy, the oldest of the sciences, has more than renewed her

youth. At no time in the past has she been so bright with unbounded aspirations and hopes. Never were her temples so numerous, nor the crowd of her votaries so great. The British Astronomical Association formed within the year numbers already about 600 members. Happy is the lot of those who are still on the eastern side of life's meridian!

Already, alas! the original founders of the newer methods are falling out—Kirchhoff, Ångström, D'Arrest, Secchi, Draper, Becquerel; but their places are more than filled; the pace of the race is gaining, but the goal is not and never will be in sight.

Since the time of Newton our knowledge of the phenomena of Nature has wonderfully increased, but man asks, perhaps more earnestly now than in his days, what is the ultimate reality behind the reality of the perceptions? Are they only the pebbles of the beach with which we have been playing? Does not the ocean of ultimate reality and truth lie beyond?

British Association for the Advancement of Science.

NOTTINGHAM, 1893.

ADDRESS

J. S. BURDON-SANDERSON,

M.A., M.D., LL.D., D.C.L., F.R.S., F.R.S.E, Professor of Physiology
in the University of Oxford,

PRESIDENT.

WE are assembled this evening as representatives of the sciences—men and women who seek to advance knowledge by scientific methods. The common ground on which we stand is that of belief in the paramount value of the end for which we are striving, of its inherent power to make men wiser, happier, and better; and our common purpose is to strengthen and encourage one another in our efforts for its attainment. We have come to learn what progress has been made in departments of knowledge which lie outside of our own special scientific interests and occupations, to widen our views, and to correct whatever misconceptions may have arisen from the necessity which limits each of us to his own field of study; and, above all, we are here for the purpose of bringing our divided energies into effectual and combined action.

Probably few of the members of the Association are fully aware of the influence which it has exercised during the last half-century and more in furthering the scientific development of this country. Wide as is the range of its activity, there has been no great question in the field of scientific inquiry which it has failed to discuss; no important line of investigation which it has not promoted; no great discovery which it has not welcomed. After more than sixty years of existence it still finds itself in the energy of middle life, looking back with satisfaction to what it has accomplished in its youth, and forward to an even more efficient future. One of the first of the national associations which exist in different countries for the advancement of science, its influence has been more felt than that of its successors because it is more wanted. The wealthiest

country in the world, which has profited more—vastly more—by science than any other, England stands alone in the discredit of refusing the necessary expenditure for its development, and cares not that other nations should reap the harvest for which her own sons have laboured.

It is surely our duty not to rest satisfied with the reflection that England in the past has accomplished so much, but rather to unite and agitate in the confidence of eventual success. It is not the fault of governments, but of the nation, that the claims of science are not recognised. We have against us an overwhelming majority of the community, not merely of the ignorant, but of those who regard themselves as educated, who value science only in so far as it can be turned into money; for we are still in great measure—in greater measure than any other—a nation of shopkeepers. Let us who are of the minority—the remnant who believe that truth is in itself of supreme value, and the knowledge of it of supreme utility—do all that we can to bring public opinion to our side, so that the century which has given Young, Faraday, Lyell, Darwin, Maxwell, and Thomson to England, may before it closes see us prepared to take our part with other countries in combined action for the full development of natural knowledge.

Last year the necessity of an imperial observatory for physical science was, as no doubt many are aware, the subject of a discussion in Section A, which derived its interest from the number of leading physicists who took part in it, and especially from the presence and active participation of the distinguished man who is at the head of the National Physical Laboratory at Berlin. The equally pressing necessity for a central institution for chemistry, on a scale commensurate with the practical importance of that science, has been insisted upon in this Association and elsewhere by distinguished chemists. As regards biology I shall have a word to say in the same direction this evening. Of these three requirements it may be that the first is the most pressing. If so, let us all, whatever branch of science we represent, unite our efforts to realise it, in the assurance that if once the claim of science to liberal public support is admitted, the rest will follow.

In selecting a subject on which to address you this evening I have followed the example of my predecessors in limiting myself to matters more or less connected with my own scientific occupations, believing that in discussing what most interests myself I should have the best chance of interesting you. The circumstance that at the last meeting of the British Association in this town, Section D assumed for the first time the title which it has since held, that of the Section of Biology, suggested to me that I might take the word 'biology' as my starting-point, giving you some account of its origin and first use, and of the relations which subsist between biology and other branches of natural science.

ORIGIN AND MEANING OF THE TERM 'BIOLOGY.'

The word 'biology,' which is now so familiar as comprising the sum of the knowledge which has as yet been acquired concerning living nature, was unknown until after the beginning of the present century. The term was first employed by Treviranus, who proposed to himself as a life-task the development of a new science, the aim of which should be to study the forms and phenomena of life, its origin and the conditions and laws of its existence, and embodied what was known on these subjects in a book of seven volumes, which he entitled 'Biology, or the Philosophy of Living Nature.' For its construction the material was very scanty, and was chiefly derived from the anatomists and physiologists. For botanists were entirely occupied in completing the work which Linnæus had begun, and the scope of zoology was in like manner limited to the description and classification of animals. It was a new thing to regard the study of living nature as a science by itself, worthy to occupy a place by the side of natural philosophy, and it was therefore necessary to vindicate its claim to such a position. Treviranus declined to found this claim on its useful applications to the arts of agriculture and medicine, considering that to regard any subject of study in relation to our bodily wants—in other words to utility—was to narrow it, but dwelt rather on its value as a discipline and on its surpassing interest. He commends biology to his readers as a study which, above all others, 'nourishes and maintains the taste for simplicity and nobleness; which affords to the intellect ever new material for reflection, and to the imagination an inexhaustible source of attractive images.'

Being himself a mathematician as well as a naturalist, he approaches the subject both from the side of natural philosophy and from that of natural history, and desires to found the new science on the fundamental distinction between living and non-living material. In discussing this distinction, he takes as his point of departure the constancy with which the activities which manifest themselves in the universe are balanced, emphasising the impossibility of excluding from that balance the vital activities of plants and animals. The difference between vital and physical processes he accordingly finds, not in the nature of the processes themselves, but in their co-ordination; that is, in their adaptedness to a given purpose, and to the peculiar and special relation in which the organism stands to the external world. All of this is expressed in a proposition difficult to translate into English, in which he defines life as consisting in the reaction of the organism to external influences, and contrasts the uniformity of vital reactions with the variety of their exciting causes.¹

¹ 'Leben besteht in der Gleichförmigkeit der Reaktionen bei ungleichförmiger Einwirkungen der Aussenwelt.'—Treviranus, *Biologie oder Philosophie der lebenden Natur*, Göttingen, 1802, vol. i. p. 83.

The purpose which I have in view in taking you back as I have done to the beginning of the century is not merely to commemorate the work done by the wonderfully acute writer to whom we owe the first scientific conception of the science of life as a whole, but to show that this conception, as expressed in the definition I have given you as its foundation, can still be accepted as true. It suggests the *idea of organism* as that to which all other biological ideas must relate. It also suggests, although perhaps it does not express it, that *action* is not an attribute of the organism but of its essence—that if, on the one hand, protoplasm is the basis of life, life is the basis of protoplasm. Their relations to each other are reciprocal. We think of the visible structure only in connection with the invisible process. The definition is also of value as indicating at once the two lines of inquiry into which the science has divided by the natural evolution of knowledge. These two lines may be easily educed from the general principle from which Treviranus started, according to which it is the fundamental characteristic of the organism that all that goes on in it is to the advantage of the whole. I need scarcely say that this fundamental conception of organism has at all times presented itself to the minds of those who have sought to understand the distinction between living and non-living. Without going back to the true father and founder of biology, Aristotle, we may recall with interest the language employed in relation to it by the physiologists of three hundred years ago. It was at that time expressed by the term *consensus partium*—which was defined as the concurrence of parts in action, of such a nature that each does *quod suum est*, all combining to bring about one effect ‘as if they had been in secret council,’ but at the same time *constanti quadam naturæ lege*.¹ Professor Huxley has made familiar to us how a century later Descartes imagined to himself a mechanism to carry out this *consensus*, based on such scanty knowledge as was then available of the structure of the nervous system. The discoveries of the early part of the present century relating to reflex action and the functions of sensory and motor nerves, served to realise in a wonderful way his anticipations as to the channels of influence, afferent and efferent, by which the *consensus* is maintained; and in recent times (as we hope to learn from Professor Horsley’s lecture on the physiology of the nervous system) these channels have been investigated with extraordinary minuteness and success.

Whether with the old writers we speak about *consensus*, with Treviranus about *adaptation*, or are content to take *organism* as our point of departure, it means that, regarding a plant or an animal as an organism, we concern ourselves primarily with its activities or, to use the word which best expresses it, its energies. Now the first thing that strikes us in beginning to think about the activities of an organism is that they are naturally

¹ Bausner, *De Consensu Partium Humani Corporis*, Amst., 1556, Præf. ad lectorem, p. 4.

distinguishable into two kinds, according as we consider the action of the whole organism in its relation to the external world or to other organisms, or the action of the parts or organs in their relation to each other. The distinction to which we are thus led between the *internal* and *external* relations of plants and animals has of course always existed, but has only lately come into such prominence that it divides biologists more or less completely into two camps—on the one hand those who make it their aim to investigate the actions of the organism and its parts by the accepted methods of physics and chemistry, carrying this investigation as far as the conditions under which each process manifests itself will permit; on the other those who interest themselves rather in considering the place which each organism occupies, and the part which it plays in the economy of nature. It is apparent that the two lines of inquiry, although they equally relate to what the organism *does*, rather than to what it *is*, and therefore both have equal right to be included in the one great science of life, or biology, yet lead in directions which are scarcely even parallel. So marked, indeed, is the distinction, that Professor Haeckel some twenty years ago proposed to separate the study of organisms with reference to their place in nature under the designation of ‘*œcology*,’ defining it as comprising ‘the relations of the animal to its organic as well as to its inorganic environment, particularly its friendly or hostile relations to those animals or plants with which it comes into direct contact.’¹ Whether this term expresses it or not, the distinction is a fundamental one. Whether with the *œcologist* we regard the organism in relation to the world, or with the *physiologist* as a wonderful complex of vital energies, the two branches have this in common, that both studies fix their attention, not on stuffed animals, butterflies in cases, or even microscopical sections of the animal or plant body—all of which relate to the framework of life—but on life itself.

The conception of biology which was developed by Treviranus as far as the knowledge of plants and animals which then existed rendered possible, seems to me still to express the scope of the science. I should have liked, had it been within my power, to present to you both aspects of the subject in equal fulness; but I feel that I shall best profit by the present opportunity if I derive my illustrations chiefly from the division of biology to which I am attached—that which concerns the *internal* relations of the organism, it being my object not to specialise in either direction, but, as Treviranus desired to do, to regard it as part—surely a very important part—of the great science of nature.

The origin of life, the first transition from non-living to living, is a

¹ These he identifies with ‘those complicated mutual relations which Darwin designates as conditions of the struggle for existence.’ Along with chorology—the distribution of animals—*œcology* constitutes what he calls *Relations-physiologie*. Haeckel, ‘Entwicklungsgang u. Aufgaben der Zoologie,’ *Jenaische Zeitschr.* vol. v. 1869, p. 353.

riddle which lies outside of our scope. No seriously-minded person, however, doubts that organised nature as it now presents itself to us has become what it is by a process of gradual perfecting or advancement, brought about by the elimination of those organisms which failed to obey the fundamental principle of adaptation which Treviranus indicated. Each step, therefore, in this evolution is a reaction to external influences, the motive of which is essentially the same as that by which from moment to moment the organism governs itself. And the whole process is a necessary outcome of the fact that those organisms are most prosperous which look best after their own welfare. As in that part of biology which deals with the internal relations of the organism, the interest of the individual is in like manner the sole motive by which every energy is guided. We may take what Treviranus called *selfish* adaptation—*Zweckmässigkeit für sich selber*—as a connecting link between the two branches of biological study. Out of this relation springs another which I need not say was not recognised until after the Darwinian epoch—that, I mean, which subsists between the two evolutions, that of the race and that of the individual. Treviranus, no less distinctly than his great contemporary Lamarck, was well aware that the affinities of plants and animals must be estimated according to their developmental value, and consequently that classification must be founded on development; but it occurred to no one what the real link was between descent and development; nor was it, indeed, until several years after the publication of the ‘Origin’ that Haeckel enunciated that ‘biogenetic law’ according to which the development of any individual organism is but a memory, a recapitulation by the individual of the development of the race—of the process for which Fritz Müller had coined the excellent word ‘phylogenesis’; and that each stage of the former is but a transitory reappearance of a bygone epoch in its ancestral history. If, therefore, we are right in regarding ontogenesis as dependent on phylogenesis the origin of the former must correspond with that of the latter; that is, on the power which the race or the organism at every stage of its existence possesses of profiting by every condition or circumstance for its own advancement.

From the short summary of the connection between different parts of our science you will see that biology naturally falls into three divisions, and these are even more sharply distinguished by their methods than by their subjects; namely, *Physiology*, of which the methods are entirely experimental; *Morphology*, the science which deals with the forms and structure of plants and animals, and of which it may be said that the body is anatomy, the soul, development; and finally *Œcology*, which uses all the knowledge it can obtain from the other two, but chiefly rests on the exploration of the endless varied phenomena of animal and plant life as they manifest themselves under natural conditions. This last branch of biology—the science which concerns itself with the external relations of

plants and animals to each other, and to the past and present conditions of their existence—is by far the most attractive. In it those qualities of mind which especially distinguish the naturalist find their highest exercise, and it represents more than any other branch of the subject what Treviranus termed the ‘philosophy of living nature.’ Notwithstanding the very general interest which several of its problems excite at the present moment I do not propose to discuss any of them, but rather to limit myself to the humbler task of showing that the fundamental idea which finds one form of expression in the world of living beings regarded as a whole—the prevalence of the best—manifests itself with equal distinctness, and plays an equally essential part in the internal relations of the organism in the great science which treats of them—Physiology.

ORIGIN AND SCOPE OF MODERN PHYSIOLOGY.

Just as there was no true philosophy of living nature until Darwin, we may with almost equal truth say that physiology did not exist as a science before Johannes Müller. For although the sum of his numerous achievements in comparative anatomy and physiology, notwithstanding their extraordinary number and importance, could not be compared for merit and fruitfulness with the one discovery which furnished the key to so many riddles, he, no less than Darwin, by his influence on his successors was the beginner of a new era.

Müller taught in Berlin from 1833 to 1857. During that time a gradual change was in progress in the way in which biologists regarded the fundamental problem of life. Müller himself, in common with Treviranus and all the biological teachers of his time, was a vitalist, *i.e.*, he regarded what was then called the *vis vitalis*—the *Lebenskraft*—as something capable of being correlated with the physical forces; and as a necessary consequence held that phenomena should be classified or distinguished, according to the forces which produced them, as vital or physical, and that all those processes—that is groups or series of phenomena in living organisms—for which, in the then very imperfect knowledge which existed, no obvious physical explanation could be found, were sufficiently explained when they were stated to be dependent on so-called vital laws. But during the period of Müller’s greatest activity times were changing, and he was changing with them. During his long career as professor at Berlin he became more and more objective in his tendencies, and exercised an influence in the same direction on the men of the next generation, teaching them that it was better and more useful to observe than to philosophise; so that, although he himself is truly regarded as the last of the vitalists—for he was a vitalist to the last—his successors were adherents of what has been very inadequately designated the mechanistic view of the phenomena of life. The change thus brought about just before the middle of this century was a revolution. It was not a substitution of one point of view for another, but simply a frank aban-

donment of theory for fact, of speculation for experiment. Physiologists ceased to theorise because they found something better to do. May I try to give you a sketch of this era of progress?

Great discoveries as to the structure of plants and animals had been made in the course of the previous decade, those especially which had resulted from the introduction of the microscope as an instrument of research. By its aid Schwann had been able to show that all organised structures are built up of those particles of living substance which we now call cells, and recognise as the seats and sources of every kind of vital activity. Hugo Mohl, working in another direction, had given the name 'protoplasm' to a certain hyaline substance which forms the lining of the cells of plants, though no one as yet knew that it was the essential constituent of all living structures—the basis of life no less in animals than in plants. And, finally, a new branch of study—histology—founded on observations which the microscope had for the first time rendered possible, had come into existence. Bowman, one of the earliest and most successful cultivators of this new science, called it physiological anatomy,¹ and justified the title by the very important inferences as to the secreting function of epithelial cells and as to the nature of muscular contraction, which he deduced from his admirable anatomical researches. From structure to function, from microscopical observation to physiological experiment, the transition was natural. Anatomy was able to answer some questions, but asked many more. Fifty years ago physiologists had microscopes but had no laboratories. English physiologists—Bowman, Paget, Sharpey—were at the same time anatomists, and in Berlin Johannes Müller, along with anatomy and physiology, taught comparative anatomy and pathology. But soon that specialisation which, however much we may regret its necessity, is an essential concomitant of progress, became more and more inevitable. The structural conditions on which the processes of life depend had become, if not known, at least accessible to investigation; but very little indeed had been ascertained of the nature of the processes themselves—so little indeed that if at this moment we could blot from the records of physiology the whole of the information which had been acquired, say in 1840, the loss would be difficult to trace—not that the previously known facts were of little value, but because every fact of moment has since been subjected to experimental verification. It is for this reason that, without any hesitation, we accord to Müller and to his successors Brücke, du Bois-Reymond, Helmholtz, who were his pupils, and Ludwig, in Germany, and to Claude Bernard² in France, the title of founders of our science. For it is

¹ The first part of the *Physiological Anatomy* appeared in 1843. It was concluded in 1856.

² It is worthy of note that these five distinguished men were nearly contemporaries: Ludwig graduated in 1839, Bernard in 1843, the other three between those dates. Three survive—Helmholtz, Ludwig, du Bois-Reymond.

the work which they began at that remarkable time (1845-55), and which is now being carried on by their pupils or their pupils' pupils in England, America, France, Germany, Denmark, Sweden, Italy, and even in that youngest contributor to the advancement of science, Japan, that physiology has been gradually built up to whatever completeness it has at present attained.

What were the conditions which brought about this great advance which coincided with the middle of the century? There is but little difficulty in answering the question. I have already said that the change was not one of doctrine, but of method. There was, however, a leading idea in the minds of those who were chiefly concerned in bringing it about. That leading notion was, that, however complicated may be the conditions under which vital energies manifest themselves, they can be split into processes which are identical in nature with those of the non-living world, and, as a corollary to this, that the analysing of a vital process into its physical and chemical constituents, so as to bring these constituents into measurable relation with physical or chemical standards, is the only mode of investigating them which can lead to satisfactory results.

There were several circumstances which at that time tended to make the younger physiologists (and all of the men to whom I have just referred were then young) sanguine, perhaps too sanguine, in the hope that the application of experimental methods derived from the exact sciences would afford solutions of many physiological problems. One of these was the progress which had been made in the science of chemistry, and particularly the discovery that many of the compounds which before had been regarded as special products of vital processes could be produced in the laboratory, and the more complete knowledge which had been thereby acquired of their chemical constitutions and relations. In like manner, the new school profited by the advances which had been made in physics, partly by borrowing from the physical laboratory various improved methods of observing the phenomena of living beings, but chiefly in consequence of the direct bearing of the crowning discovery of that epoch (that of the conservation of energy) on the discussions which then took place as to the relations between vital and physical forces; in connection with which it may be noted that two of those who (along with Mr. Joule and your President at the last Nottingham meeting) took a prominent part in that discovery—Helmholtz and J. R. Mayer—were physiologists as much as they were physicists. I will not attempt even to enumerate the achievements of that epoch of progress. I may, however, without risk of wearying you, indicate the lines along which research at first proceeded, and draw your attention to the contrast between then and now. At present a young observer who is zealous to engage in research finds himself provided with the most elaborate means of investigation, the chief obstacle to his success being that the problems which have

been left over by his predecessors are of extreme difficulty, all of the easier questions having been worked out. There were then also difficulties, but of an entirely different kind. The work to be done was in itself easier, but the means for doing it were wanting, and every investigator had to depend on his own resources. Consequently the successful men were those who, in addition to scientific training, possessed the ingenuity to devise and the skill to carry out methods for themselves. The work by which du Bois-Reymond laid the foundation of animal electricity would not have been possible had not its author, besides being a trained physicist, known how to do as good work in a small room in the upper floor of the old University Building at Berlin as any which is now done in his splendid laboratory. Had Ludwig not possessed mechanical aptitude, in addition to scientific knowledge, he would have been unable to devise the apparatus by which he measured and recorded the variations of arterial pressure (1848), and verified the principles which Young had laid down thirty years before as to the mechanics of the circulation. Nor, lastly, could Helmholtz, had he not been a great deal more than a mere physiologist, have made those measurements of the time-relations of muscular and nervous responses to stimulation, which not only afford a solid foundation for all that has been done since in the same direction, but have served as models of physiological experiment, and as evidence that perfect work was possible and was done by capable men, even when there were no physiological laboratories.

Each of these examples relates to work done within a year or two of the middle of the century.¹ If it were possible to enter more fully on the scientific history of the time, we should, I think, find the clearest evidence, first, that the foundation was laid in anatomical discoveries, in which it is gratifying to remember that English anatomists (Allen Thomson, Bowman, Goodsir, Sharpey) took considerable share; secondly, that progress was rendered possible by the rapid advances which, during the previous decade, had been made in physics and chemistry, and the participation of physiology in the general awakening of the scientific spirit which these discoveries produced. I venture, however, to think that, notwithstanding the operation of these two causes, or rather combinations of causes, the development of our science would have been delayed had it not been for the exceptional endowments of the four or five young experimenters whose names I have mentioned, each of whom was capable of becoming a master in his own branch, and of guiding the future progress of inquiry.

Just as the affinities of an organism can be best learned from its development, so the scope of a science may be most easily judged of by

¹ The *Untersuchungen über thierische Electricität* appeared in 1848; Ludwig's researches on the circulation, which included the first description of the 'kymograph' and served as the foundation of the 'graphic method' in 1847; Helmholtz's research on the propagation in motor nerves in 1851

the tendencies which it exhibits in its origin. I wish now to complete the sketch I have endeavoured to give of the way in which physiology entered on the career it has since followed for the last half-century, by a few words as to the influence exercised on general physiological theory by the progress of research. We have seen that no real advance was made until it became possible to investigate the phenomena of life by methods which approached more or less closely to those of the physicist, in exactitude. The methods of investigation being physical or chemical, the organism itself naturally came to be considered as a complex of such processes, and nothing more. And in particular the idea of adaptation, which, as I have endeavoured to show, is not a consequence of organism, but its essence, was in great measure lost sight of. Not, I think, because it was any more possible than before to conceive of the organism otherwise than as a working together of parts for the good of the whole, but rather that, if I may so express it, the minds of men were so occupied with new facts that they had not time to elaborate theories. The old meaning of the term 'adaptation' as the equivalent of 'design' had been abandoned, and no new meaning had yet been given to it, and consequently the word 'mechanism' came to be employed as the equivalent of 'process,' as if the constant concomitance or sequence of two events was in itself a sufficient reason for assuming a mechanical relation between them. As in daily life so also in science, the misuse of words leads to misconceptions. To assert that the link between *a* and *b* is mechanical, for no better reason than that *b* always follows *a*, is an error of statement, which is apt to lead the incautious reader or hearer to imagine that the relation between *a* and *b* is understood, when in fact its nature may be wholly unknown. Whether or not at the time which we are considering, some physiological writers showed a tendency to commit this error, I do not think that it found expression in any generally accepted theory of life. It may, however, be admitted that the rapid progress of experimental investigation led to too confident anticipations, and that to some enthusiastic minds it appeared as if we were approaching within measurable distance of the end of knowledge. Such a tendency is, I think, a natural result of every signal advance. In an eloquent Harveian oration, delivered last autumn by Dr. Bridges, it was indicated how, after Harvey's great discovery of the circulation, men were too apt to found upon it explanations of all phenomena whether of health or disease, to such an extent that the practice of medicine was even prejudicially affected by it. In respect of its scientific importance the epoch we are considering may well be compared with that of Harvey, and may have been followed by an undue preference of the new as compared with the old, but no more permanent unfavourable results have shown themselves. As regards the science of medicine we need only remember that it was during the years between 1845 and 1860 that Virchow made those researches by which he brought the processes of disease into immediate relation with the normal processes

of cell-development and growth, and so, by making pathology a part of physiology, secured its subsequent progress and its influence on practical medicine. Similarly in physiology, the achievements of those years led on without any interruption or drawback to those of the following generation; while in general biology, the revolution in the mode of regarding the internal processes of the animal or plant organism which resulted from these achievements, prepared the way for the acceptance of the still greater revolution which the Darwinian epoch brought about in the views entertained by naturalists of the relations of plants and animals to each other and to their surroundings.

It has been said that every science of observation begins by going out botanising, by which, I suppose, is meant that collecting and recording observations is the first thing to be done in entering on a new field of inquiry. The remark would scarcely be true of physiology, even at the earliest stage of its development, for the most elementary of its facts could scarcely be picked up as one gathers flowers in a wood. Each of the processes which go to make up the complex of life requires separate investigation, and in each case the investigation must consist in first splitting up the process into its constituent phenomena, and then determining their relation to each other, to the process of which they form part, and to the conditions under which they manifest themselves. It will, I think, be found that even in the simplest inquiry into the nature of vital processes some such order as this is followed. Thus, for example, if muscular contraction be the subject on which we seek information, it is obvious that, in order to measure its duration, the mechanical work it accomplishes, the heat wasted in doing it, the electro-motive forces which it develops, and the changes of form associated with these phenomena, special modes of observation must be used for each of them, that each measurement must be in the first instance separately made, under special conditions, and by methods specially adapted to the required purpose. In the synthetic part of the inquiry the guidance of experiment must again be sought for the purpose of discriminating between apparent and real causes, and of determining the order in which the phenomena occur. Even the simplest experimental investigations of vital processes are beset with difficulties. For, in addition to the extreme complexity of the phenomena to be examined and the uncertainties which arise from the relative inconstancy of the conditions of all that goes on in the living organism, there is this additional drawback, that, whereas in the exact sciences experiment is guided by well-ascertained laws, here the only principle of universal application is that of adaptation, and that even this cannot, like a law of physics, be taken as a basis for deductions, but only as a summary expression of that relation between external exciting causes and the reactions to which they give rise, which, in accordance with Treviranus' definition, is the essential character of vital activity.

THE SPECIFIC ENERGIES OF THE ORGANISM.

When in 1826 J. Müller was engaged in investigating the physiology of vision and hearing he introduced into the discussion a term, 'specific energy,' the use of which by Helmholtz¹ in his physiological writings has rendered it familiar to all students. Both writers mean by the word energy, not the 'capacity of doing work,' but simply *activity*, using it in its old-fashioned meaning, that of the Greek word from which it is derived. With the qualification 'specific' it serves, perhaps, better than any other expression to indicate the way in which adaptation manifests itself. In this more extended sense the 'specific energy' of a part or organ—whether that part be a secreting cell, a motor cell of the brain or spinal cord, or one of the photogenous cells which produce the light of the glowworm, or the protoplasmic plate which generates the discharge of the torpedo—is simply the special action which it *normally* performs, its normal or rule of action being in each instance the *interest of the organism* as a whole of which it forms part, and the exciting cause some influence outside of the excited structure, technically called a stimulus. It thus stands for a characteristic of living structures which seems to be universal. The apparent exceptions are to be found in those bodily activities which, following Bichat, we call vegetative, because they go on, so to speak, as a matter of course; but the more closely we look into them the more does it appear that they form no exception to the general rule, that every link in the chain of living action, however uniform that action may be, is a response to an antecedent influence. Nor can it well be doubted that, as every living cell or tissue is called upon to act in the interest of the whole, the organism must be capable of influencing every part so as to regulate its action. For, although there are some instances in which the channels of this influence are as yet unknown, the tendency of recent investigations has been to diminish the number of such instances. In general there is no difficulty in determining both the nature of the central influence exercised and the relation between it and the normal function. It may help to illustrate this relation to refer to the expressive word *Auslösung* by which it has for many years been designated by German writers. This word stands for the performance of function by the 'letting off' of 'specific energies.' Carrying out the notion of 'letting off' as expressing the link between action and reaction, we might compare the whole process to the mode of working of a repeating clock (or other similar mechanism), in which case the pressure of the finger on the button would represent the external influence or stimulus, the striking of the clock, the normal reaction. And now may I ask you to consider in detail one or two illustrations of physiological reaction—of the *letting off of specific energy*?

¹ *Handb. der physiologischen Optik*, 1886, p. 233. Helmholtz uses the word in the plural—the 'energies of the nerves of special sense.'

The repeater may serve as a good example, inasmuch as it is, in biological language, a highly differentiated structure, to which a single function is assigned. So also in the living organism, we find the best examples of specific energy where Müller found them, namely, in the most differentiated, or, as we are apt to call them, the *highest* structures. The retina, with the part of the brain which belongs to it, together constitute such a structure, and will afford us therefore the illustration we want, with this advantage for our present purpose, that the phenomena are such as we all have it in our power to observe in ourselves. In the visual apparatus the principle of *normality* of reaction is fully exemplified. In the physical sense the word 'light' stands for ether vibrations, but in the sensuous or subjective sense for sensations. The swings are the stimulus, the sensations are the reaction. Between the two comes the link, the 'letting off,' which it is our business to understand. Here let us remember that the man who first recognised this distinction between the physical and the physiological was not a biologist, but a physicist. It was Young who first made clear (though his doctrine fell on unappreciative ears) that, although in vision the external influences which give rise to the sensation of light are infinitely varied, the responses need not be more than three in number, each being, in Müller's language, a 'specific energy' of some part of the visual apparatus. We speak of the organ of vision as *highly differentiated*, an expression which carries with it the suggestion of a distinction of rank between different vital processes. The suggestion is a true one; for it would be possible to arrange all those parts or organs of which the bodies of the *higher* animals consist in a series, placing at the lower end of the series those of which the functions are continuous, and therefore called vegetative; at the other, those highly specialised structures, as, *e.g.*, those in the brain, which in response to physical light produce physiological, that is subjective, light; or, to take another instance, the so-called motor cells of the surface of the brain, which in response to a stimulus of much greater complexity produce voluntary motion. And just as in civilised society an individual is valued according to his power of doing one thing well, so the high rank which is assigned to the structure, or rather to the 'specific energy' which it represents, belongs to it by virtue of its specialisation. And if it be asked how this conformity is manifested, the answer is, by the quality, intensity, duration, and extension of the response, in all which respects vision serves as so good an example, that we can readily understand how it happened that it was in this field that the relation between response and stimulus was first clearly recognised. I need scarcely say that, however interesting it might be to follow out the lines of inquiry thus indicated, we cannot attempt it this evening. All that I can do is to mention one or two recent observations which, while they serve as illustrations, may perhaps be sufficiently novel to interest even those who are at home in the subject.

Probably everyone is acquainted with some of the familiar proofs that an object is seen for a much longer period than it is actually exposed to view; that the visual reaction lasts much longer than its cause. More precise observations teach us that this response is regulated according to laws which it has in common with all the higher functions of an organism. If, for example, the cells in the brain of the torpedo are 'let off'—that is, awakened by an external stimulus—the electrical discharge, which, as in the case of vision, follows after a certain interval, lasts a certain time, first rapidly increasing to a maximum of intensity, then more slowly diminishing. In like manner, as regards the visual apparatus, we have, in the response to a sudden invasion of the eye by light, a rise and fall of a similar character. In the case of the electrical organ, and in many analogous instances, it is easy to investigate the time relations of the successive phenomena, so as to represent them graphically. Again, it is found that in many physiological reactions, the period of rising 'energy' (as Helmholtz called it) is followed by a period during which the responding structure is not only inactive, but its capacity for energising is so completely lost that the same exciting cause which a moment before 'let off' the characteristic response is now without effect. As regards vision, it has long been believed that these general characteristics of physiological reaction have their counterpart in the visual process, the most striking evidence being that in the contemplation of a lightning flash—or, better, of an instantaneously illuminated white disc¹—the eye seems to receive a double stroke, indicating that, although the stimulus is single and instantaneous, the response is reduplicated. The most precise of the methods we until lately possessed for investigating the wax and wane of the visual reaction, were not only difficult to carry out but left a large margin of uncertainty. It was therefore particularly satisfactory when M. Charpentier, of Nancy, whose merits as an investigator are perhaps less known than they deserve to be, devised an experiment of extreme simplicity which enables us, not only to observe, but to measure with great facility both phases of the reaction. It is difficult to explain even the simplest apparatus without diagrams; you will, however, understand the experiment if you will imagine that you are contemplating a disc, like those ordinarily used for colour mixing; that it is divided by two radial lines which diverge from each other at an angle of 60°; that the sector which these lines enclose is white, the rest black; that the disc revolves slowly, about once in two seconds. You then see, close to the front edge of the advancing sector, a black bar, followed by a second at the same distance from itself but much fainter. Now the scientific value of the experiment consists in this, that the angular distance of the bar from the black border is in proportion to the frequency of the revolutions—the faster the wider. If, for

¹ The phenomenon is best seen when, in a dark room, the light of a luminous spark is thrown on to a white screen with the aid of a suitable lens.

example, when the disc makes half a revolution in a second the distance is ten degrees, this obviously means that when light bursts into the eye, the extinction happens one-eighteenth of a second after the excitation.¹

The fact thus demonstrated, that the visual reaction consequent on an instantaneous illumination exhibits the alternations I have described, has enabled M. Charpentier to make out another fact in relation to the visual reaction which is, I think, of equal importance. In all the instances, excepting the retina, in which the physiological response to stimulus has a definite time-limitation, and in so far resembles an explosion—in other words, in all the higher forms of specific energy, it can be shown experimentally that the process is propagated from the part first directly acted on to other contiguous parts of similar endowment. Thus in the simplest of all known phenomena of this kind, the electrical change, by which the leaf of the *Dionæa* plant responds to the slightest touch of its sensitive hairs, is propagated from one side of the leaf to the other, so that in the opposite lobe the response occurs after a delay which is proportional to the distance between the spot excited and the spot observed. That in the retina there is also such propagation has not only been surmised from analogy, but inferred from certain observed facts. M. Charpentier has now been able by a method which, although simple, I must not attempt to describe, not only to prove its existence, but to measure its rate of progress over the visual field.

There is another aspect of the visual response to the stimulus of light which, if I am not trespassing too long on your patience, may, I think, be interesting to consider. As the relations between the sensations of colour and the physical properties of the light which excites them, are among the most certain and invariable in the whole range of vital reactions, it is obvious that they afford as fruitful a field for physiological investigation as those in which white light is concerned. We have on one side physical facts, that is, wave-lengths or vibration-rates; on the other, facts in consciousness—namely, sensations of colour—so simple that notwithstanding their subjective character there is no difficulty in measuring either their intensity or their duration. Between these there are *lines of influence*, neither physical nor psychological, which pass from the former to the latter through the visual apparatus (retina, nerve, brain). It is these lines of influence which interest the physiologist. The structure of the visual apparatus affords us no clues to trace them by. The most important fact we know about them is that they must be at least three in number.

It has been lately assumed by some that vision, like every other specific energy, having been developed progressively, objects were seen

¹ Charpentier, 'Réaction oscillatoire de la Rétine sous l'influence des excitations lumineuses,' *Archives de Physiol.*, vol. xxiv. p. 541, and *Propagation de l'action oscillatoire*, &c., p. 362.

by the most elementary forms of eye only in chiaroscuro, that afterwards some colours were distinguished, eventually all. As regards hearing it is so. The organ which, on structural grounds, we consider to represent that of hearing in animals low in the scale of organisation—as, *e.g.*, in the Ctenophora—has nothing to do with sound,¹ but confers on its possessor the power of judging of the direction of its own movements in the water in which it swims, and of guiding these movements accordingly. In the lowest vertebrates, as, *e.g.*, in the dogfish, although the auditory apparatus is much more complicated in structure, and plainly corresponds with our own, we still find the particular part which is concerned in hearing scarcely traceable. All that is provided for is that sixth sense, which the higher animals also possess, and which enables them to judge of the direction of their own movements. But a stage higher in the vertebrate series we find the special mechanisms by which we ourselves appreciate sounds beginning to appear—not supplanting or taking the place of the imperfect organ, but added to it. As regards hearing, therefore, a new function is acquired without any transformation or fusion of the old into it. We ourselves possess the sixth sense, by which we keep our balance and which serves as the guide to our bodily movements. It resides in the part of the internal ear which is called the labyrinth. At the same time we enjoy along with it the possession of the cochlea, that more complicated apparatus by which we are able to hear sounds and to discriminate their vibration-rates.

As regards vision, evidence of this kind is wanting. There is, so far as I know, no proof that visual organs which are so imperfect as to be incapable of distinguishing the forms of objects, may not be affected differently by their colours. Even if it could be shown that the least perfect forms of eye possess only the power of discriminating between light and darkness, the question whether in our own such a faculty exists separately from that of distinguishing colours is one which can only be settled by experiment. As in all sensations of colour the sensation of brightness is mixed, it is obvious that one of the first points to be determined is whether the latter represents a 'specific energy' or merely a certain combination of specific energies which are excited by colours. The question is not whether there is such a thing as white light, but whether we possess a separate faculty by which we judge of light and shade—a question which, although we have derived our knowledge of it chiefly from physical experiment, is one of eye and brain, not of wavelengths or vibration-rates, and is therefore essentially physiological.

There is a German proverb which says, 'Bei Nacht sind alle Katzen grau.' The fact which this proverb expresses presents itself experimentally when a spectrum projected on a white surface is watched, while the

¹ Verworn, 'Gleichgewicht u. Otolithenorgan,' *Pflüger's Archiv*, vol. 1., p. 423; also Ewald's Researches on the Labyrinth as a Sense-organ (*Ueber das Endorgan des Nervus octavus*, Wiesbaden, 1892).

intensity of the light is gradually diminished. As the colours fade away they become indistinguishable as such, the last seen being the primary red and green. Finally they also disappear, but a grey band of light still remains, of which the most luminous part is that which before was green.¹ Without entering into details, let us consider what this tells us of the specific energy of the visual apparatus. Whether or not the faculty by which we see grey in the dark is one which we possess in common with animals of imperfectly developed vision, there seems little doubt that there are individuals of our own species who, in the fullest sense of the expression, have no eye for colour; in whom all colour sense is absent; persons who inhabit a world of grey, seeing all things as they might have done had they and their ancestors always lived nocturnal lives. In the theory of colour vision, as it is commonly stated, no reference is made to such a faculty as we are now discussing.

Professor Hering, whose observations as to the diminished spectrum I referred to just now, who was among the first to subject the vision of the *totally* colour-blind to accurate examination, is of opinion, on that and on other grounds, that the sensation of light and shade is a specific faculty. Very recently the same view has been advocated on a wide basis by a distinguished psychologist, Professor Ebbinghaus.² Happily, as regards the actual experimental results relating to both these main subjects, there seems to be a complete coincidence of observation between observers who interpret them differently. Thus the recent elaborate investigations of Captain Abney³ (with General Festing), representing graphically the results of his measurements of the subjective values of the different parts of the diminished spectrum, as well as those of the fully illuminated spectrum as seen by the totally colour-blind, are in the closest accord with the observations of Hering, and have, moreover, been substantially confirmed in both points by the measurements of Dr. König in Helmholtz' laboratory at Berlin.⁴ That observers of such eminence as the three persons whom I have mentioned, employing different methods and with a different purpose in view, and without reference to each other's work, should arrive in so complicated an inquiry at coincident results, augurs well for the speedy settlement of this long-debated question. At present the inference seems to be that such a specific energy as Hering's theory of vision postulates actually exists, and that it has for associates the colour-perceiving activities of the visual apparatus, provided that these are present; but that whenever the intensity

¹ Hering, 'Untersuch. eines total Farbenblinden,' *Pflüger's Arch.*, vol. xlix, 1891, p. 563.

² Ebbinghaus, 'Theorie des Farbensehens,' *Zeitschr. f. Psychol.*, vol. v., 1893, p. 145.

³ Abney and Festing, Colour Photometry, Part III. *Phil. Trans.*, vol. clxxxiii.A, 1891, p. 531.

⁴ König, 'Ueber den Helligkeitwerth der Spectralfarben bei verschiedener absoluter Intensität,' *Beiträge zur Psychologie*, &c., 'Festschrift zu H. von Helmholtz, 70. Geburtstage,' 1891, p. 309.

of the illumination is below the chromatic threshold—that is, too feeble to awaken these activities—or when, as in the totally colour-blind, they are wanting, it manifests itself independently; all of which can be most easily understood on such a hypothesis as has lately been suggested in an ingenious paper by Mrs. Ladd Franklin,¹ that each of the elements of the visual apparatus is made up of a central structure for the sensation of light and darkness, with collateral appendages for the sensations of colour—it being, of course, understood that this is a mere diagrammatic representation, which serves no purposes beyond that of facilitating the conception of the relation between the several ‘specific energies.’

EXPERIMENTAL PSYCHOLOGY.

Resisting the temptation to pursue this subject further, I will now ask you to follow me into a region which, although closely connected with the subjects we have been considering, is beset with greater difficulties—the subject in which, under the name of Physiological or Experimental Psychology, physiologists and psychologists have of late years taken a common interest—a borderland not between fact and fancy, but between two methods of investigation of questions which are closely related, which here, though they do not overlap, at least interdigitate. It is manifest that, quite irrespectively of any foregone conclusion as to the dependence of mind on processes of which the biologist is accustomed to take cognizance, mind must be regarded as one of the ‘specific energies’ of the organism, and should on that ground be included in the subject-matter of physiology. As, however, our science, like other sciences, is limited not merely by its subject but also by its method, it actually takes in only so much of psychology as is experimental. Thus sensation, although it is psychological, and the investigation of its relation to the special structures by which the mind keeps itself informed of what goes on in the outside world, have always been considered to be in the physiological sphere. And it is by anatomical researches relating to the minute structure and to the development of the brain, by observation of the facts of disease, and, above all, by physiological experiment, that those changes in the ganglion cells of the brain and spinal cord which are the immediate antecedents of every kind of bodily action have been traced. Between the two—that is, between sensation and the beginning of action—there is an intervening region which the physiologist has hitherto willingly resigned to psychology, feeling his incompetence to use the only instrument by which it can be explored—that of introspection. This consideration enables us to understand the course which the new study (I will not claim for it the title of a new science, regarding it as merely a part of the great science of life) has hitherto

¹ Christine Ladd Franklin, ‘Eine neue Theorie der Lichtempfindungen,’ *Zeitschr. für Psychologie*, vol. iv., 1893, p. 211; see also the Proceedings of the last Psychological Congress in London, 1892.

followed, and why physiologists have been unwilling to enter on it. The study of the less complicated internal relations of the organism has afforded so many difficult problems that the most difficult of all have been deferred; so that although the psycho-physical method was initiated by E. H. Weber in the middle of the present century, by investigations¹ which formed part of the work done at that epoch of discovery, and although Professor Wundt, also a physiologist, has taken a larger share in the more recent development of the new study, it is chiefly by psychologists that the researches which have given to it its importance as a new discipline have been conducted.

Although, therefore, experimental psychology has derived its methods from physical science, the result has been not so much that physiologists have become philosophers, as that philosophers have become experimental psychologists. In our own universities, in those of America, and still more in those of Germany, psychological students of mature age are to be found who are willing to place themselves in the dissecting-room side by side with beginners in anatomy, in order to acquire that exact knowledge of the framework of the organism without which no man can understand its working. Those, therefore, who are apprehensive lest the regions of mind should be invaded by the *insaniens sapientia* of the laboratory, may, I think, console themselves with the thought that the invaders are for the most part men who before they became laboratory workers had already given their allegiance to philosophy; their purpose being not to relinquish definitively, but merely to lay aside for a time, the weapons in the use of which they had been trained, in order to learn the use of ours. The motive that has encouraged them has not been any hope of finding an experimental solution of any of the ultimate problems of philosophy, but the conviction that, inasmuch as the relation between mental stimuli and the mental processes which they awaken is of the same order with the relation between every other vital process and its specific determinant, the only hope of ascertaining its nature must lie in the employment of the same methods of comparative measurement which the biologist uses for similar purposes. Not that there is necessarily anything scientific in mere measurement, but that measurement affords the only means by which it can be determined whether or not the same conformity in the relation between stimulus and reaction which we have accepted as the fundamental characteristic of life, is also to be found in mind, notwithstanding that mental processes have no known physical concomitants. The results of experimental psychology tend to show that it is so, and consequently that in so far the processes in question are as truly functions of organism as the contraction of a muscle, or as the changes produced in the retinal pigment by light.

I will make no attempt even to enumerate the special lines of inquiry

¹ Weber's researches were published in Wagner's *Handwörterbuch*, I think, in 1849.

which during the last decade have been conducted with such vigour in all parts of the world, all of them traceable to the influence of the Leipzig school; but will content myself with saying that the general purpose of these investigations has been to determine with the utmost attainable precision the nature of psychical relations. Some of these investigations begin with those simpler reactions which more or less resemble those of an automatic mechanism, proceeding to those in which the resulting action or movement is modified by the influence of auxiliary or antagonistic conditions, or changed by the simultaneous or antecedent action on the reagent of other stimuli, in all of which cases the effect can be expressed quantitatively; others lead to results which do not so readily admit of measurement. In pursuing this course of inquiry the physiologist finds himself as he proceeds more and more the *coadjutor* of the psychologist, less and less his *director*; for whatever advantage the former may have in the mere *technique* of observation, the things with which he has to do are revealed only to introspection, and can be studied only by methods which lie outside of his sphere. I might in illustration of this refer to many recent experimental researches—such, for example, as those by which it has been sought to obtain exact data as to the physiological concomitants of pleasure and of pain, or as to the influence of weariness and recuperation, as modifiers of psychological reactions. Another outwork of the mental citadel which has been invaded by the experimental method is that of memory. Even here it can be shown that in the comparison of transitory as compared with permanent memory—as, for example, in the getting off by heart of a wholly uninteresting series of words, with subsequent oblivion and reacquisition—the labour of acquiring and reacquiring may be measured, and consequently the relation between them; and that this ratio varies according to a simple numerical law.

I think it not unlikely that the only effect of what I have said may be to suggest to some of my hearers the question, What is the use of such inquiries? Experimental psychology has, to the best of my knowledge, no technical application. The only satisfactory answer I can give is that it has exercised, and will exercise in future, a helpful influence on the science of life. Every science of observation, and each branch of it, derives from the peculiarities of its methods certain tendencies which are apt to predominate unduly. We speak of this as specialisation, and are constantly striving to resist its influence. The most successful way of doing so is by availing ourselves of the counter-acting influence which two opposite tendencies mutually exercise when they are simultaneous. He that is skilled in the methods of introspection naturally (if I may be permitted to say so) looks at the same thing from an opposite point of view to that of the experimentalist. It is, therefore, good that the two should so work together that the tendency of the experimentalist to imagine the existence of mechanism where none

is proved to exist—of the psychologist to approach the phenomena of mind too exclusively from the subjective side—may mutually correct and assist each other.

PHOTOTAXIS AND CHEMIOTAXIS.

Considering that every organism must have sprung from a unicellular ancestor, some have thought that unless we are prepared to admit a deferred epigenesis of mind, we must look for psychical manifestations even among the lowest animals, and that as in the protozoon all the vital activities are blended together, mind should be present among them not merely potentially but actually, though in diminished degree.

Such a hypothesis involves ultimate questions which it is unnecessary to enter upon: it will, however, be of interest in connection with our present subject to discuss the phenomena which served as a basis for it—those which relate to what may be termed the behaviour of unicellular organisms and of individual cells, in so far as these last are capable of reacting to external influences. The observations which afford us most information are those in which the stimuli employed can be easily measured, such as electrical currents, light, or chemical agents in solution.

A single instance, or at most two, must suffice to illustrate the influence of light in directing the movements of freely moving cells, or, as it is termed, phototaxis. The rod-like purple organism called by Engelmann *Bacterium photometricum*,¹ is such a light-lover that if you place a drop of water containing these organisms under the microscope, and focus the smallest possible beam of light on a particular spot in the field, the spot acts as a light trap and becomes so crowded with the little rodlets as to acquire a deep port-wine colour. If instead of making his trap of white light, he projected on the field a microscopic spectrum, Engelmann found that the rodlets showed their preference for a spectral colour which is absorbed when transmitted through their bodies. By the aid of a light trap of the same kind, the very well-known spindle-shaped and flagellate cell of *Euglena* can be shown to have a similar power of discriminating colour, but its preference is different. This familiar organism advances with its flagellum forwards, the sharp end of the spindle having a red or orange eye point. Accordingly, the light it loves is again that which is most absorbed—viz., the blue of the spectrum (line F).

These examples may serve as an introduction to a similar one in which the directing cause of movement is not physical but chemical. The spectral light trap is used in the way already described; the or-

¹ Engelmann, 'Bacterium photometricum,' *Onderzoek. Physiol. Lab. Utrecht*, vol. vii. p. 200; also Ueber Licht- u. Farbenperception niederster Organismen, *Pflügers Arch.*, vol. xxix. p. 387.

ganisms to be observed are not coloured, but bacteria of that common sort which twenty years ago we used to call *Bacterium termo*, and which is recognised as the ordinary determining cause of putrefaction. These organisms do not care for light, but are great oxygen-lovers. Consequently, if you illuminate with your spectrum a filament of a confervoid alga, placed in water containing bacteria, the assimilation of carbon and consequent disengagement of oxygen is most active in the part of the filament which receives the red rays (B to C). To this part, therefore, where there is a dark band of absorption, the bacteria which want oxygen are attracted in crowds. The motive which brings them together is their desire for oxygen. Let us compare other instances in which the source of attraction is food.

The plasmodia of the myxomycetes, particularly one which has been recently investigated by Mr. Arthur Lister,¹ may be taken as a typical instance of what may be called the chemical allurements of living protoplasm. In this organism, which in the active state is an expansion of labile living material, the delicacy of the reaction is comparable to that of the sense of smell in those animals in which the olfactory organs are adapted to an aquatic life. Just as, for example, the dogfish is attracted by food which it cannot see, so the plasmodium of *Badhamia* becomes aware, as if it smelled it, of the presence of its food—a particular kind of fungus. I have no diagram to explain this, but will ask you to imagine an expansion of living material, quite structureless, spreading itself along a wet surface; that this expansion of transparent material is bounded by an irregular coast-line; and that somewhere near the coast there has been placed a fragment of the material on which the *Badhamia* feeds. The presence of this bit of *Stereum* produces an excitement at the part of the plasmodium next to it. Towards this centre of activity streams of living material converge. Soon the afflux leads to an outgrowth of the plasmodium, which in a few minutes advances towards the desired fragment, envelopes, and incorporates it.

May I give you another example also derived from the physiology of plants? Very shortly after the publication of Engelmann's observations of the attraction of bacteria by oxygen, Pfeffer made the remarkable discovery that the movements of the antherozoids of ferns and of mosses are guided by impressions derived from chemical sources, by the allurements exercised upon them by certain chemical substances in solution—in one of the instances mentioned by sugar, in the other by an organic acid. The method consisted in introducing the substance to be tested, in any required strength, into a minute capillary tube closed at one end, and placing it under the microscope in water inhabited by antherozoids, which thereupon showed their predilection for the substance, or the contrary, by its effect on their movements. In accordance with the

¹ Lister, 'On the Plasmodium of *Badhamia utricularis*, &c.,' *Annals of Botany*, No. 5 June 1888.

principle followed in experimental psychology, Pfeffer¹ made it his object to determine, not the relative effects of different doses, but the smallest perceptible increase of dose which the organism was able to detect, with this result—that, just as in measurements of the relation between stimulus and reaction in ourselves we find that the sensational value of a stimulus depends, not on its absolute intensity, but on the ratio between that intensity and the previous excitation, so in this simplest of vital reagents the same so-called psycho-physical law manifests itself. It is not, however, with a view to this interesting relation that I have referred to Pfeffer's discovery, but because it serves as a centre around which other phenomena, observed alike in plants and animals, have been grouped. As a general designation of reactions of this kind Pfeffer devised the term Chemotaxis, or, as we in England prefer to call it, Chemiotaxis. Pfeffer's contrivance for chemiotactic testing was borrowed from the pathologists, who have long used it for the purpose of determining the relation between a great variety of chemical compounds or products, and the colourless corpuscles of the blood. I need, I am sure, make no apology for referring to a question which, although purely pathological, is of very great biological interest—the theory of the process by which, not only in man, but also, as Metschnikoff has strikingly shown, in animals far down in the scale of development, the organism protects itself against such harmful things as, whether particulate or not, are able to penetrate its framework. Since Cohnheim's great discovery in 1867 we have known that the central phenomenon of what is termed by pathologists *inflammation* is what would now be called a chemiotactic one; for it consists in the gathering together, like that of vultures to a carcase, of those migratory cells which have their home in the blood stream and in the lymphatic system, to any point where the living tissue of the body has been injured or damaged, as if the products of disintegration which are set free where such damage occurs were attractive to them.

The fact of chemiotaxis, therefore, as a constituent phenomenon of the process of inflammation, was familiar in pathology long before it was understood. Cohnheim himself attributed it to changes in the channels along which the cells moved, and this explanation was generally accepted, though some writers, at all events, recognised its incompleteness. But no sooner was Pfeffer's discovery known than Leber,² who for years had been working at the subject from the pathological side, at once saw that the two processes were of similar nature. Then followed a variety of researches of great interest, by which the importance of chemiotaxis in relation to the destruction of disease-producing microphytes was proved,

¹ Pfeffer, *Untersuch. a. d. botan. Institute z. Tübingen*, vol. i., part 3, 1884.

² Leber, 'Die Anhäufung der Leucocyten am Orte des Entzündungsreizes,' &c. *Die Entstehung der Entzündung*, &c., pp. 423–464, Leipzig, 1891.

by that of Buchner¹ on the chemical excitability of leucocytes being among the most important. Much discussion has taken place, as many present are aware, as to the kind of wandering cells, or leucocytes, which in the first instance attack morbid microbes, and how they deal with them. The question is not by any means decided. It has, however, I venture to think, been conclusively shown that the process of destruction is a chemical one, that the destructive agent has its source in the chemiotactic cells—that is, cells which act under the orders of chemical stimuli. Two Cambridge observers, Messrs. Kanthack and Hardy,² have lately shown that, in the particular instance which they have investigated, the cells which are most directly concerned in the destruction of morbid *bacilli*, although chemiotactic, do not possess the power of incorporating either bacilli or particles of any other kind. While, therefore, we must regard the relation between the process of devitalising and that of incorporating as not yet sufficiently determined, it is now no longer possible to regard the latter as essential to the former.

There seems, therefore, to be very little doubt that chemiotactic cells are among the agents by which the human or animal organism protects itself against infection. There are, however, many questions connected with this action which have not yet been answered. The first of these are chemical ones—that of the nature of the attractive substance and that of the process by which the living carriers of infection are destroyed. Another point to be determined is how far the process admits of adaptation to the particular infection which is present in each case, and to the state of liability or immunity of the infected individual. The subject is therefore of great complication. None of the points I have suggested can be settled by experiments in glass tubes such as I have described to you. These serve only as indications of the course to be followed in much more complicated and difficult investigations—when we have to do with acute diseases as they actually affect ourselves or animals of similar liabilities to ourselves, and find ourselves face to face with the question of their causes.

It is possible that many members of the Association are not aware of the unfavourable—I will not say discreditable—position that this country at present occupies in relation to the scientific study of this great subject—the causes and mode of prevention of infectious diseases. As regards administrative efficiency in matters relating to public health England was at one time far ahead of all other countries, and still retains its superiority; but as regards scientific knowledge we are, in this subject as in others, content to borrow from our neighbours. Those who desire either to learn the methods of research or to carry out scientific

¹ Buchner, 'Die chem. Reizbarkeit der Leucocyten,' &c., *Berliner klin. Woch.*, 1890, No. 17.

² Kanthack and Hardy, 'On the Characters and Behaviour of the Wandering Cells of the Frog,' *Proceedings of the Royal Society*, vol. lli., p. 267.

inquiries have to go to Berlin, to Munich, to Breslau, or to the Pasteur Institute in Paris to obtain what England ought long ago to have provided. For to us, from the spread of our race all over the world, the prevention of acute infectious diseases is more important than to any other nation. At the beginning of this address I urged the claims of pure science. If I could, I should feel inclined to speak even more strongly of the application of science to the discovery of the causes of acute diseases. May I express the hope that the effort which is now being made to establish in England an Institution for this purpose not inferior in efficiency to those of other countries, may have the sympathy of all present? And now may I ask your attention for a few moments more to the subject that more immediately concerns us?

CONCLUSION.

The purpose which I have had in view has been to show that there is one principle—that of adaptation—which separates biology from the exact sciences, and that in the vast field of biological inquiry the end we have is not merely, as in natural philosophy, to investigate the relation between a phenomenon and the antecedent and concomitant conditions on which it depends, but to possess this knowledge in constant reference to the interest of the organism. It may perhaps be thought that this way of putting it is too teleological, and that in taking, as it were, as my text this evening so old-fashioned a biologist as Treviranus, I am yielding to a retrogressive tendency. It is not so. What I have desired to insist on is that *organism* is a fact which encounters the biologist at every step in his investigations; that in referring it to any general biological principle, such as adaptation, we are only referring it to itself, not explaining it; that no explanation will be attainable until the conditions of its coming into existence can be subjected to experimental investigation so as to correlate them with those of processes in the non-living world.

Those who were present at the meeting of the British Association at Liverpool will remember that then, as well as at some subsequent meetings, the question whether the conditions necessary for such an inquiry could be realised was a burning one. This is no longer the case. The patient endeavours which were made about that time to obtain experimental proof of what was called *abiogenesis*, although they conduced materially to that better knowledge which we now possess of the conditions of life of bacteria, failed in the accomplishment of their purpose. The question still remains undetermined; it has, so to speak, been adjourned *sine die*. The only approach to it lies at present in the investigation of those rare instances in which, although the relations between a living organism and its environment ceases as a watch stops when it

has not been wound, these relations can be re-established—the process of life re-awakened—by the application of the required stimulus.

I was also desirous to illustrate the relation between physiology and its two neighbours on either side, natural philosophy (including chemistry) and psychology. As regards the latter I need add nothing to what has already been said. As regards the former, it may be well to notice that although physiology can never become a mere branch of applied physics or chemistry, there are parts of physiology wherein the principles of these sciences may be applied directly. Thus, in the beginning of the century, Young applied his investigations as to the movements of liquids in a system of elastic tubes, directly to the phenomena of the circulation; and a century before, Borelli successfully examined the mechanisms of locomotion and the action of muscles, without reference to any, excepting mechanical principles. Similarly, the foundation of our present knowledge of the process of nutrition was laid in the researches of Bidder and Schmidt, in 1851, by determinations of the weight and composition of the body, the daily gain of weight by food or oxygen, the daily loss by the respiratory and other discharges, all of which could be accomplished by chemical means. But in by far the greater number of physiological investigations, both methods (the physical or chemical and the physiological) must be brought to bear on the same question—to co-operate for the elucidation of the same problem. In the researches, for example, which during several years have occupied Professor Bohr, of Copenhagen, relating to the exchange of gases in respiration, he has shown that factors purely physical—namely, the partial pressures of oxygen and carbon dioxide in the blood which flows through the pulmonary capillaries—are, so to speak, interfered with in their action by the ‘specific energy’ of the pulmonary tissue, in such a way as to render this fundamental process, which, since Lavoisier, has justly been regarded as one of the most important in physiology, much more complicated than we for a long time supposed it to be. In like manner Heidenhain has proved that the process of lymphatic absorption, which before we regarded as dependent on purely mechanical causes—*i.e.*, differences of pressure—is in great measure due to the specific energy of cells, and that in various processes of secretion the principal part is not, as we were inclined not many years ago to believe, attributable to liquid diffusion, but to the same agency. I wish that there had been time to have told you something of the discoveries which have been made in this particular field by Mr. Langley, who has made the subject of ‘specific energy’ of secreting-cells his own. It is in investigations of this kind, of which any number of examples could be given, in which vital reactions mix themselves up with physical and chemical ones so intimately that it is difficult to draw the line between them, that the physiologist derives most aid from whatever chemical and physical training he may be fortunate enough to possess.

There is, therefore, no doubt as to the advantages which physiology derives from the exact sciences. It could scarcely be averred that they would benefit in anything like the same degree from closer association with the science of life. Nevertheless, there are some points in respect of which that science may have usefully contributed to the advancement of physics or of chemistry. The discovery of Graham as to the characters of colloid substances, and as to the diffusion of bodies in solution through membranes, would never have been made had not Graham 'ploughed,' so to speak, 'with our heifer.' The relations of certain colouring matters to oxygen and carbon dioxide would have been unknown, had no experiments been made on the respiration of animals and the assimilative process in plants; and, similarly, the vast amount of knowledge which relates to the chemical action of ferments must be claimed as of physiological origin. So also there are methods, both physical and chemical, which were originally devised for physiological purposes. Thus the method by which meteorological phenomena are continuously recorded graphically, originated from that used by Ludwig (1847) in his 'Researches on the Circulation'; the mercurial pump, invented by Lothar Meyer, was perfected in the physiological laboratories of Bonn and Leipzig; the rendering the galvanometer needle aperiodic by damping was first realised by du Bois-Reymond—in all of which cases invention was prompted by the requirements of physiological research.

Let me conclude with one more instance of a different kind, which may serve to show how, perhaps, the wonderful ingenuity of contrivance which is displayed in certain organised structures—the eye, the ear, or the organ of voice—may be of no less interest to the physicist than to the physiologist. Johannes Müller, as is well known, explained the compound eye of insects on the theory that an erect picture is formed on the convex retina by the combination of pencils of light, received from different parts of the visual field through the eyelets (ommatidia) directed to them. Years afterwards it was shown that in each eyelet an image is formed which is reversed. Consequently, the mosaic theory of Müller was for a long period discredited on the ground that an erect picture could not be made up of 'upside-down' images. Lately the subject has been reinvestigated, with the result that the mosaic theory has regained its authority. Professor Exner¹ has proved photographically that behind each part of the insect's eye an erect picture is formed of the objects towards which it is directed. There is, therefore, no longer any difficulty in understanding how the whole field of vision is mapped out as consistently as it is imaged on our own retina, with the difference, of course, that the picture is erect. But behind this fact lies a physical question—that of the relation between the erect picture which is photographed and the optical structure of the crystal cones which produce it—

¹ Exner, *Die Physiologie der facettirten Augen von Krebsen u. Insecten*, Leipzig, 1891.

a question which, although we cannot now enter upon it, is quite as interesting as the physiological one.

With this history of a theory which, after having been for thirty years disbelieved, has been reinstated by the fortunate combination of methods derived from the two sciences, I will conclude. It may serve to show how, though physiology can never become a part of natural philosophy, the questions we have to deal with are cognate. Without forgetting that every phenomenon has to be regarded with reference to its useful purpose in the organism, the aim of the physiologist is not to inquire into final causes, but to investigate processes. His question is ever *How*, rather than *Why*.

May I illustrate this by a simple, perhaps too trivial, story, which derives its interest from its having been told of the childhood of one of the greatest natural philosophers of the present century? ¹ He was even then possessed by that insatiable curiosity which is the first quality of the investigator; and it is related of him that his habitual question was ‘What is the *go* of it?’ and if the answer was unsatisfactory, ‘What is the particular *go* of it?’ That North Country boy became Professor Clerk Maxwell. The questions he asked are those which in our various ways we are all trying to answer.

¹ *Life of Clerk Maxwell* (Campbell and Garnett), p. 28.

British Association for the Advancement of Science.

IPSWICH, 1895.

ADDRESS

BY

SIR DOUGLAS GALTON, K.C.B., D.C.L., F.R.S.,
PRESIDENT.

My first duty is to convey to you, Mr. Mayor, and to the inhabitants of Ipswich, the thanks of the British Association for your hospitable invitation to hold our sixty-fifth meeting in your ancient town, and thus to recall the agreeable memories of the similar favour which your predecessors conferred on the Association forty-four years ago.

In the next place I feel it my duty to say a few words on the great loss which science has recently sustained—the death of the Right Hon. Thomas Henry Huxley. It is unnecessary for me to enlarge, in the presence of so many to whom his personality was known, upon his charm in social and domestic life; but upon the debt which the Association owes to him for the assistance which he rendered in the promotion of science I cannot well be silent. Huxley was preeminently qualified to assist in sweeping away the obstruction by dogmatic authority, which in the early days of the Association fettered progress in certain branches of science. For, whilst he was an eminent leader in biological research, his intellectual power, his original and intrepid mind, his vigorous and masculine English, made him a writer who explained the deepest subject with transparent clearness. And as a speaker his lucid and forcible style was adorned with ample and effective illustration in the lecture-room; and his energy and wealth of argument in a more public arena largely helped to win the battle of evolution, and to secure for us the right to discuss questions of religion and science without fear and without favour.

It may, I think, interest you to learn that Huxley first made the

acquaintance of Tyndall at the meeting of the Association held in this town in 1851.

About forty-six years ago I first began to attend the meetings of the British Association; and I was elected one of your general secretaries about twenty-five years ago.

It is not unfitting, therefore, that I should recall to your minds the conditions under which science was pursued at the formation of the Association, as well as the very remarkable position which the Association has occupied in relation to science in this country.

Between the end of the sixteenth century and the early part of the present century several societies had been created to develop various branches of science. Some of these societies were established in London, and others in important provincial centres.

In 1831, in the absence of railways, communication between different parts of the country was slow and difficult. Science was therefore localised; and in addition to the universities in England, Scotland, and Ireland, the towns of Birmingham, Manchester, Plymouth and York each maintained an important nucleus of scientific research.

ORIGIN OF THE BRITISH ASSOCIATION.

Under these social conditions the British Association was founded in September 1831.

The general idea of its formation was derived from a migratory society which had been previously formed in Germany; but whilst the German society met for the special occasion on which it was summoned, and then dissolved, the basis of the British Association was continuity.

The objects of the founders of the British Association were enunciated in their earliest rules to be:—

‘To give a stronger impulse and a more systematic direction to scientific inquiry; to promote the intercourse of those who cultivated science in different parts of the British Empire with one another, and with foreign philosophers; to obtain a more general attention to the objects of science, and a removal of any disadvantages of a public kind which impede its progress.’

Thus the British Association for the Advancement of Science based its utility upon the opportunity it afforded for combination.

The first meeting of the Association was held at York with 353 members.

As an evidence of the want which the Association supplied, it may be mentioned that at the second meeting, which was held at Oxford, the number of members was 435. The third meeting, at Cambridge, numbered over 900 members, and at the meeting at Edinburgh in 1834 there were present 1,298 members.

At its third meeting, which was held at Cambridge in 1833, the Association, through the influence it had already acquired, induced the

Government to grant a sum of 500*l.* for the reduction of the astronomical observations of Baily. And at the same meeting the General Committee commenced to appropriate to scientific research the surplus from the subscriptions of its members. The committees on each branch of science were desired 'to select definite and important objects of science, which they may think most fit to be advanced by an application of the funds of the society, either in compensation for labour, or in defraying the expense of apparatus, or otherwise, stating their reasons for their selection, and, when they may think proper, designating individuals to undertake the desired investigations.'

The several proposals were submitted to the Committee of Recommendations, whose approval was necessary before they could be passed by the General Committee. The regulations then laid down still guide the Association in the distribution of its grants. At that early meeting the Association was enabled to apply 600*l.* to these objects.

I have always wondered at the foresight of the framers of the constitution of the British Association, the most remarkable feature of which is the lightness of the tie which holds it together. It is not bound by any complex central organisation. It consists of a federation of Sections, whose youth and energy are yearly renewed by a succession of presidents and vice-presidents, whilst in each Section some continuity of action is secured by the less movable secretaries.

The governing body is the General Committee, the members of which are selected for their scientific work ; but their controlling power is tempered by the law that all changes of rules, or of constitution, should be submitted to, and receive the approval of, the Committee of Recommendations. This committee may be described as an ideal Second Chamber. It consists of the most experienced members of the Association.

The administration of the Association in the interval between annual meetings is carried on by the Council, an executive body, whose duty it is to complete the work of the annual meeting (*a*) by the publication of its proceedings ; (*b*) by giving effect to resolutions passed by the General Committee ; (*c*) it also appoints the Local Committee and organises the *personnel* of each Section for the next meeting.

I believe that one of the secrets of the long-continued success and vitality of the British Association lies in this purely democratic constitution, combined with the compulsory careful consideration which must be given to suggested organic changes.

The Association is now in the sixty-fifth year of its existence. In its origin it invited the philosophical societies dispersed throughout Great Britain to unite in a co-operative union.

Within recent years it has endeavoured to consolidate that union.

At the present time almost all important local scientific societies scattered throughout the country, some sixty-six in number, are in correspondence with the Association. Their delegates hold annual conferences at our meetings. The Association has thus extended the sphere of its action:

it places the members of the local societies engaged in scientific work in relation with each other, and brings them into co-operation with members of the Association and with others engaged in original investigations, and the papers which the individual societies publish annually are catalogued in our Report. Thus by degrees a national catalogue will be formed of the scientific work of these societies.

The Association has, moreover, shown that its scope is coterminous with the British Empire by holding one of its annual meetings at Montreal, and we are likely soon to hold a meeting in Toronto.

CONDITION OF CERTAIN SCIENCES AT THE FORMATION OF THE BRITISH ASSOCIATION.

The Association, at its first meeting, began its work by initiating a series of reports upon the then condition of the several sciences.

A rapid glance at some of these reports will not only show the enormous strides which have been made since 1831 in the investigation of facts to elucidate the laws of Nature, but it may afford a slight insight into the impediments offered to the progress of investigation by the mental condition of the community, which had been for so long satisfied to accept assumptions without undergoing the labour of testing their truth by ascertaining the real facts. This habit of mind may be illustrated by two instances selected from the early reports made to the Association. The first is afforded by the report made in 1832, by Mr. Lubbock, on 'Tides.'

This was a subject necessarily of importance to England as a dominant power at sea. But in England records of the tides had only recently been commenced at the dockyards of Woolwich, Sheerness, Portsmouth, and Plymouth, on the request of the Royal Society, and no information had been collected upon the tides on the coasts of Scotland and Ireland.

The British Association may feel pride in the fact that within three years of its inception, viz. by 1834, it had induced the Corporation of Liverpool to establish two tide gauges, and the Government to undertake tidal observations at 500 stations on the coasts of Britain.

Another cognate instance is exemplified by a paper read at the second meeting, in 1832, upon the State of Naval Architecture in Great Britain. The author contrasts the extreme perfection of the carpentry of the internal fittings of the vessels with the remarkable deficiency of mathematical theory in the adjustment of the external form of vessels, and suggests the benefit of the application of refined analysis to the various practical problems which ought to interest shipbuilders—problems of capacity, of displacement, of stowage, of velocity, of pitching and rolling, of masting, of the effects of sails and of the resistance of fluids; and, moreover, suggests that large-scale experiments should be made by Government, to afford the necessary data for calculation.

Indeed, when we consider how completely the whole habit of mind of the populations of the Western world has been changed, since the beginning

of the century, from willing acceptance of authority as a rule of life to a universal spirit of inquiry and experimental investigation, is it not probable that this rapid change has arisen from society having been stirred to its foundations by the causes and consequences of the French Revolution?

One of the earliest practical results of this awakening in France was the conviction that the basis of scientific research lay in the accuracy of the standards by which observations could be compared; and the following principles were laid down as a basis for their measurements of length, weight, and capacity: viz. (1) that the unit of linear measure applied to matter in its three forms of extension, viz., length, breadth, and thickness, should be the standard of measures of length, surface, and solidity; (2) that the cubic contents of the linear measure in decimetres of pure water at the temperature of its greatest density should furnish at once the standard weight and the measure of capacity.¹ The metric system did not come into full operation in France till 1840; and it is now adopted by all countries on the continent of Europe except Russia.

The standards of length which were accessible in Great Britain at the formation of the Association were the Parliamentary standard yard lodged in the Houses of Parliament (which was destroyed in 1834 in the fire which burned the Houses of Parliament); the Royal Astronomical Society's standard; and the 10-foot bar of the Ordnance Survey.

The first two were assumed to afford exact measurements at a given temperature. The Ordnance bar was formed of two bars on the principle of a compensating pendulum, and afforded measurements independent of temperature. Standard bars were also disseminated throughout the country, in possession of the corporations of various towns.

The British Association early recognised the importance of uniformity in the record of scientific facts, as well as the necessity for an easy method of comparing standards and for verifying differences between instruments and apparatus required by various observers pursuing similar lines of investigation. At its meeting at Edinburgh in 1834 it caused a comparison to be made between the standard bar at Aberdeen, constructed by Troughton, and the standard of the Royal Astronomical Society, and reported that the scale 'was exceedingly well finished; it was about $\frac{1}{500}$ th of an inch shorter than the 5-feet of the Royal Astronomical Society's scale, but it was evident that a great number of minute, yet important, circumstances have hitherto been neglected in the formation of such scales, without an attention to which they cannot be expected to accord with that degree of accuracy which the present state of science demands.' Subsequently, at the meeting at Newcastle in 1863, the Association appointed a committee, to report on the best means of providing for a uniformity of weights and measures with reference to the

¹ The litre is the volume of a kilogramme of pure water at its maximum density, and is slightly less than the litre was intended to be, viz., one cubic decimetre. The weight of a cubic decimetre of pure water is 1.000013 kilogrammes.

interests of science. This committee recommended the metric decimal system—a recommendation which has been endorsed by a committee of the House of Commons in the last session of Parliament.

British instrument-makers had been long conspicuous for accuracy of workmanship. Indeed, in the eighteenth century practical astronomy had been mainly in the hands of British observers ; for although the mathematicians of France and other countries on the continent of Europe were occupying the foremost place in mathematical investigation, means of astronomical observation had been furnished almost exclusively by English artisans.

The sectors, quadrants, and circles of Ramsden, Bird, and Cary were inimitable by Continental workmen.

But the accuracy of the mathematical-instrument maker had not penetrated into the engineer's workshop. And the foundation of the British Association was coincident with a rapid development of mechanical appliances.

At that time a good workman had done well if the shaft he was turning, or the cylinder he was boring, 'was right to the $\frac{3}{32}$ nd of an inch.' This was, in fact, a degree of accuracy as fine as the eye could usually distinguish.

Few mechanics had any distinct knowledge of the method to be pursued for obtaining accuracy ; nor, indeed, had practical men sufficiently appreciated either the immense importance or the comparative facility of its acquisition.

The accuracy of workmanship essential to this development of mechanical progress required very precise measurements of length, to which reference could be easily made. No such standards were then available for the workshops. But a little before 1830 a young workman named Joseph Whitworth realised that the basis of accuracy in machinery was the making of a true plane. The idea occurred to him that this could only be secured by making three independent plane surfaces ; if each of these would lift the other, they must be planes, and they must be true.

The true plane rendered possible a degree of accuracy beyond the wildest dreams of his contemporaries in the construction of the lathe and the planing machine, which are used in the manufacture of all tools.

His next step was to introduce an exact system of measurement, generally applicable in the workshop.

Whitworth felt that the eye was altogether inadequate to secure this, and appealed to the sense of touch for affording a means of comparison. If two plugs be made to fit into a round hole, they may differ in size by a quantity imperceptible to the eye, or to any ordinary process of measurement, but in fitting them into the hole the difference between the larger and the smaller is felt immediately by the greater ease with which the smaller one fits. In this way a child can tell which is the larger of two cylinders differing in thickness by no more than $\frac{1}{8000}$ th of an inch.

Standard gauges, consisting of hollow cylinders with plugs to fit, but

differing in diameter by the $\frac{1}{10000}$ th or the $\frac{1}{100000}$ th of an inch, were given to his workmen, with the result that a degree of accuracy inconceivable to the ordinary mind became the rule of the shop.

To render the construction of accurate gauges possible Whitworth devised his measuring machine, in which the movement was effected by a screw; by this means the distance between two true planes might be measured to the one-millionth of an inch.

These advances in precision of measurement have enabled the degree of accuracy which was formerly limited to the mathematical-instrument maker to become the common property of every machine shop. And not only is the latest form of steam-engine, in the accuracy of its workmanship, little behind the chronometer of the early part of the century, but the accuracy in the construction of experimental apparatus which has thus been introduced has rendered possible recent advances in many lines of research.

Lord Kelvin said in his Presidential Address at Edinburgh, 'Nearly all the grandest discoveries of science have been but the rewards of accurate measurement and patient, long-continued labour in the sifting of numerical results.' The discovery of argon, for which Lord Rayleigh and Professor Ramsay have been awarded the Hodgkin prize by the Smithsonian Institution affords a remarkable illustration of the truth of this remark. Indeed, the provision of accurate standards not only of length, but of weight, capacity, temperature, force, and energy, are amongst the foundations of scientific investigation.

In 1842 the British Association obtained the opportunity of extending its usefulness in this direction.

In that year the Government gave up the Royal Observatory at Kew, and offered it to the Royal Society, who declined it. But the British Association accepted the charge. Their first object was to continue Sabine's valuable observations upon the vibrations of a pendulum in various gases, and to promote pendulum observations in various parts of the world. They subsequently extended it into an observatory for comparing and verifying the various instruments which recent discoveries in physical science had suggested for continuous meteorological and magnetic observations, for observations and experiments on atmospheric electricity, and for the study of solar physics.

This new departure afforded a means for ascertaining the advantages and disadvantages of the several varieties of scientific instruments; as well as for standardising and testing instruments, not only for instrument-makers, but especially for observers by whom simultaneous observations were then being carried on in different parts of the world; and also for training observers proceeding abroad on scientific expeditions.

Its special object was to promote original research, and expenditure was not to be incurred on apparatus merely intended to exhibit the necessary consequences of known laws.

The rapid strides in electrical science had attracted attention to the

measurement of electrical resistances, and in 1859 the British Association appointed a special committee to devise a standard. The standard of resistance proposed by that committee became the generally accepted standard, until the requirements of that advancing science led to the adoption of an international standard.

In 1866 the Meteorological Department of the Board of Trade entered into close relations with the Kew Observatory.

And in 1871 Mr. Gassiot transferred 10,000*l.* upon trust to the Royal Society for the maintenance of the Kew Observatory, for the purpose of assisting in carrying on magnetical, meteorological, and other physical observations. The British Association thereupon, after having maintained this Observatory for nearly thirty years, at a total expenditure of about 12,000*l.*, handed the Observatory over to the Royal Society.

The 'Transactions' of the British Association are a catalogue of its efforts in every branch of science, both to promote experimental research and to facilitate the application of the results to the practical uses of life.

But probably the marvellous development in science which has accompanied the life-history of the Association will be best appreciated by a brief allusion to the condition of some of the branches of science in 1831 as compared with their present state.

GEOLOGICAL AND GEOGRAPHICAL SCIENCE.

Geology.

At the foundation of the Association geology was assuming a prominent position in science. The main features of English geology had been illustrated as far back as 1821, and, among the founders of the British Association, Murchison and Phillips, Buckland, Sedgwick and Conybeare, Lyell and De la Beche, were occupied in investigating the data necessary for perfecting a geological chronology by the detailed observations of the various British deposits, and by their co-relation with the Continental strata. They were thus preparing the way for those large generalisations which have raised geology to the rank of an inductive science.

In 1831 the Ordnance maps published for the southern counties had enabled the Government to recognise the importance of a geological survey by the appointment of Mr. De la Beche to affix geological colours to the maps of Devonshire and portions of Somerset, Dorset and Cornwall; and in 1835 Lyell, Buckland and Sedgwick induced the Government to establish the Geological Survey Department, not only for promoting geological science, but on account of its practical bearing on agriculture, mining, the making of roads, railways and canals, and on other branches of national industry.

Geography.

The Ordnance Survey appears to have had its origin in a proposal of the French Government to make a joint-measurement of an arc of the meridian. This proposal fell through at the outbreak of the Revolution ; but the measurement of the base for that object was taken as a foundation for a national survey. In 1831, however, the Ordnance Survey had only published the 1-inch map for the southern portion of England, and the great triangulation of the kingdom was still incomplete.

In 1834 the British Association urged upon the Government that the advancement of various branches of science was greatly retarded by the want of an accurate map of the whole of the British Isles ; and that, consequently, the engineer and the meteorologist, the agriculturist and the geologist, were each fettered in their scientific investigations by the absence of those accurate data which now lie ready to his hand for the measurement of length, of surface, and of altitude.

Yet the first decade of the British Association was coincident with a considerable development of geographical research. The Association was persistent in pressing on the Government the scientific importance of sending the expedition of Ross to the Antarctic and of Franklin to the Arctic regions. We may trust that we are approaching a solution of the geography of the North Pole ; but the Antarctic regions still present a field for the researches of the meteorologist, the geologist, the biologist, and the magnetic observer, which the recent voyage of M. Borchgrevink leads us to hope may not long remain unexplored.

In the same decade the question of an alternative route to India by means of a communication between the Mediterranean and the Persian Gulf was also receiving attention, and in 1835 the Government employed Colonel Chesney to make a survey of the Euphrates valley in order to ascertain whether that river would enable a practicable route to be formed from Iskanderoon, or Tripoli, opposite Cyprus, to the Persian Gulf. His valuable surveys are not, however, on a sufficiently extensive scale to enable an opinion to be formed as to whether a navigable waterway through Asia Minor is physically practicable, or whether the cost of establishing it might not be prohibitive.

The advances of Russia in Central Asia have made it imperative to provide an easy, rapid, and alternative line of communication with our Eastern possessions, so as not to be dependent upon the Suez Canal in time of war. If a navigation cannot be established, a railway between the Mediterranean and the Persian Gulf has been shown by the recent investigations of Messrs. Hawkshaw and Hayter, following on those of others, to be practicable, and easy of accomplishment ; such an undertaking would not only be of strategical value, but it is believed it would be commercially remunerative.

Speke and Grant brought before the Association, at its meeting at Newcastle in 1863, their solution of the mystery of the Nile basin, which

had puzzled geographers from the days of Herodotus ; and the efforts of Livingstone and Stanley and others have opened out to us the interior of Africa. I cannot refrain here from expressing the deep regret which geologists and geographers, and indeed all who are interested in the progress of discovery, feel at the recent death of Joseph Thomson. His extensive, accurate, and trustworthy observations added much to our knowledge of Africa, and by his premature death we have lost one of its most competent explorers.

CHEMICAL, ASTRONOMICAL AND PHYSICAL SCIENCE.

Chemistry.

The report made to the Association on the state of the chemical sciences in 1832, says that the efforts of investigators were then being directed to determining with accuracy the true nature of the substances which compose the various products of the organic and inorganic kingdoms, and the exact ratios by weight which the different constituents of these substances bear to each other.

But since that day the science of chemistry has far extended its boundaries. The barrier has vanished which was supposed to separate the products of living organisms from the substances of which minerals consist, or which could be formed in the laboratory. The number of distinct carbon compounds obtainable from organisms has greatly increased ; but it is small when compared with the number of such compounds which have been artificially formed. The methods of analysis have been perfected. The physical, and especially the optical, properties of the various forms of matter have been closely studied, and many fruitful generalisations have been made. The form in which these generalisations would now be stated may probably change, some, perhaps, by the overthrow or disuse of an ingenious guess at Nature's workings, but more by that change which is the ordinary growth of science—namely, inclusion in some simpler and more general view.

In these advances the chemist has called the spectroscope to his aid. Indeed, the existence of the British Association has been practically coterminous with the comparatively newly developed science of spectrum analysis, for though Newton,¹ Wollaston, Fraunhofer, and Fox Talbot had worked at the subject long ago, it was not till Kirchhoff and Bunsen set a seal on the prior labours of Stokes, Ångström, and Balfour Stewart that the spectra of terrestrial elements have been mapped out and grouped ; that by its help new elements have been discovered, and that

¹ Joannes Marcus Marci, of Kronland in Bohemia, was the only predecessor of Newton who had any knowledge of the formation of a spectrum by a prism. He not only observed that the coloured rays diverged as they left the prism, but that a coloured ray did not change in colour after transmission through a prism. His book, *Thaumantias, liber de arcu cælesti deque colorum apparentium natura*, Prag, 1648, was, however, not known to Newton, and had no influence upon future discoveries.

the idea has been suggested that the various orders of spectra of the same element are due to the existence of the element in different molecular forms—allotropic or otherwise—at different temperatures.

But great as have been the advances of terrestrial chemistry through its assistance, the most stupendous advance which we owe to the spectro-scope lies in the celestial direction.

Astronomy.

In the earlier part of this century, whilst the sidereal universe was accessible to investigators, many problems outside the solar system seemed to be unapproachable.

At the third meeting of the Association, at Cambridge, in 1833, Dr. Whewell said that astronomy is not only the queen of science, but the only perfect science, which was ‘in so elevated a state of flourishing maturity that all that remained was to determine with the extreme of accuracy the consequences of its rules by the profoundest combinations of mathematics ; the magnitude of its data by the minutest scrupulousness of observation.’

But in the previous year, viz. 1832, Airy, in his report to the Association on the progress of astronomy, had pointed out that the observations of the planet Uranus could not be united in one elliptic orbit ; a remark which turned the attention of Adams to the discovery of Neptune. In his report on the position of optical science in 1832, Brewster suggested that with the assistance of adequate instruments ‘it would be possible to study the action of the elements of material bodies upon rays of artificial light, and thereby to discover the analogies between their affinities and those which produce the fixed lines in the spectra of the stars ; and thus to study the effects of the combustions which light up the suns of other systems.’

This idea has now been realised. All the stars which shine brightly enough to impress an image of the spectrum upon a photographic plate have been classified on a chemical basis. The close connection between stars and nebulae has been demonstrated ; and while on the one hand the modern science of thermodynamics has shown that the hypothesis of Kant and Laplace on stellar formation is no longer tenable, inquiry has indicated that the true explanation of stellar evolution is to be found in the gradual condensation of meteoritic particles, thus justifying the suggestions put forward long ago by Lord Kelvin and Professor Tait.

We now know that the spectra of many of the terrestrial elements in the chromosphere of the sun differ from those familiar to us in our laboratories. We begin to glean the fact that the chromospheric spectra are similar to those indicated by the absorption going on in the hottest stars, and Lockyer has not hesitated to affirm that these facts would indicate that in those localities we are in the presence of the actions of temperatures sufficiently high to break up our chemical elements into finer forms. Other

students of these phenomena may not agree in this view, and possibly the discrepancies may be due to default in our terrestrial chemistry. Still, I would recall to you that Dr. Carpenter, in his Presidential Address at Brighton in 1872, almost-censured the speculations of Frankland and Lockyer in 1868 for attributing a certain bright line in the spectrum of solar prominences (which was not identifiable with that of any known terrestrial source of light) to a hypothetical new substance which they proposed to call 'helium,' because 'it had not received that verification which, in the case of Crookes' search for thallium, was afforded by the actual discovery of the new metal.' Ramsay has now shown that this gas is present in dense minerals on earth; but we have now also learned from Lockyer that it and other associated gases are not only found with hydrogen in the solar chromosphere, but that these gases, with hydrogen, form a large percentage of the atmospheric constituents of some of the hottest stars in the heavens.

The spectroscope has also made us acquainted with the motions and even the velocities of those distant orbs which make up the sidereal universe. It has enabled us to determine that many stars, single to the eye, are really double, and many of the conditions of these strange systems have been revealed. The rate at which matter is moving in solar cyclones and winds is now familiar to us. And I may also add that quite recently his wonderful instrument has enabled Professor Keeler to verify Clerk-Maxwell's theory that the rings of Saturn consist of a marvellous company of separate moons—as it were, a cohort of courtiers revolving round their queen—with velocities proportioned to their distances from the planet.

Physics

If we turn to the sciences which are included under physics, the progress has been equally marked.

In optical science, in 1831 the theory of emission as contrasted with the undulatory theory of light was still under discussion.

Young, who was the first to explain the phenomena due to the interference of the rays of light as a consequence of the theory of waves, and Fresnel, who showed the intensity of light for any relative position of the interference-waves, both had only recently passed away.

The investigations into the laws which regulate the conduction and radiation of heat, together with the doctrine of latent and of specific heat, and the relations of vapour to air, had all tended to the conception of a material heat, or caloric, communicated by an actual flow and emission.

It was not till 1834 that improved thermometrical appliances had enabled Forbes and Melloni to establish the polarisation of heat, and thus to lay the foundation of an undulatory theory for heat similar to that which was in progress of acceptance for light.

Whewell's report, in 1832, on magnetism and electricity shows that

these branches of science were looked upon as cognate, and that the theory of two opposite electric fluids was generally accepted.

In magnetism, the investigations of Hansteen, Gauss, and Weber in Europe, and the observations made under the Imperial Academy of Russia over the vast extent of that empire, had established the existence of magnetic poles, and had shown that magnetic disturbances were simultaneous at all the stations of observation.

At their third meeting the Association urged the Government to establish magnetic and meteorological observatories in Great Britain and her colonies and dependencies in different parts of the earth, furnished with proper instruments, constructed on uniform principles, and with provisions for continued observations at those places.

In 1839 the British Association had a large share in inducing the Government to initiate the valuable series of experiments for determining the intensity, the declination, the dip, and the periodical variations of the magnetic needle which were carried on for several years, at numerous selected stations over the surface of the globe, under the directions of Sabine and Lefroy.

In England systematic and regular observations are still made at Greenwich, Kew, and Stonyhurst. For some years past similar observations by both absolute and self-recording instruments have also been made at Falmouth—close to the home of Robert Were Fox, whose name is inseparably connected with the early history of terrestrial magnetism in this country—but under such great financial difficulties that the continuance of the work is seriously jeopardised. It is to be hoped that means may be forthcoming to carry it on. Cornishmen, indeed, could find no more fitting memorial of their distinguished countryman, John Couch Adams, than by suitably endowing the magnetic observatory in which he took so lively an interest.

Far more extended observation will be needed before we can hope to have an established theory as to the magnetism of the earth. We are without magnetic observations over a large part of the Southern Hemisphere. And Professor Rücker's recent investigations tell us that the earth seems as it were alive with magnetic forces, be they due to electric currents or to variations in the state of magnetised matter; that the disturbances affect not only the diurnal movement of the magnet, but that even the small part of the secular change which has been observed, and which has taken centuries to accomplish, is interfered with by some slower agency. And, what is more important, he tells us that none of these observations stand as yet upon a firm basis, because standard instruments have not been in accord; and much labour, beyond the power of individual effort, has hitherto been required to ascertain whether the relations between them are constant or variable.

In electricity, in 1831, just at the time when the British Association was founded, Faraday's splendid researches in electricity and magnetism at the Royal Institution had begun with his discovery of magneto-

electric induction, his investigation of the laws of electro-chemical decomposition, and of the mode of electrolytical action.

But, the practical application of our electrical knowledge was then limited to the use of lightning-conductors for buildings and ships. Indeed, it may be said that the applications of electricity to the use of man have grown up side by side with the British Association.

One of the first practical applications of Faraday's discoveries was in the deposition of metals and electro-plating, which has developed into a large branch of national industry; and the dissociating effect of the electric arc, for the reduction of ores, and in other processes, is daily obtaining a wider extension.

But probably the application of electricity which is tending to produce the greatest change in our mental, and even material condition, is the electric telegraph and its sister, the telephone. By their agency not only do we learn, almost at the time of their occurrence, the events which are happening in distant parts of the world, but they are establishing a community of thought and feeling between all the nations of the world which is influencing their attitude towards each other, and, we may hope, may tend to weld them more and more into one family.

The electric telegraph was introduced experimentally in Germany in 1833, two years after the formation of the Association. It was made a commercial success by Cooke and Wheatstone in England, whose first attempts at telegraphy were made on the line from Euston to Camden Town in 1837, and on the line from Paddington to West Drayton in 1838.

The submarine telegraph to America, conceived in 1856, became a practical reality in 1861 through the commercial energy of Cyrus Field and Pender, aided by the mechanical skill of Latimer Clark, Gooch, and others, and the scientific genius of Lord Kelvin. The knowledge of electricity gained by means of its application to the telegraph largely assisted the extension of its utility in other directions.

The electric light gives, in its incandescent form, a very perfect hygienic light. Where rivers are at hand the electrical transmission of power will drive railway trains and factories economically, and might enable each artisan to convert his room into a workshop, and thus assist in restoring to the labouring man some of the individuality which the factory has tended to destroy.

In 1843 Joule described his experiments for determining the mechanical equivalent of heat. But it was not until the meeting at Oxford, in 1847, that he fully developed the law of the conservation of energy, which, in conjunction with Newton's law of the conservation of momentum, and Dalton's law of the conservation of chemical elements, constitutes a complete mechanical foundation for physical science.

Who, at the foundation of the Association, would have believed some far-seeing philosopher if he had foretold that the spectroscope would analyse the constituents of the sun and measure the motions of the stars; that we should liquefy air and utilise temperatures approaching to the

absolute zero for experimental research ; that, like the magician in the 'Arabian Nights,' we should annihilate distance by means of the electric telegraph and the telephone ; that we should illuminate our largest buildings instantaneously, with the clearness of day, by means of the electric current ; that by the electric transmission of power we should be able to utilise the Falls of Niagara to work factories at distant places ; that we should extract metals from the crust of the earth by the same electrical agency to which, in some cases, their deposition has been attributed ?

These discoveries and their applications have been brought to their present condition by the researches of a long line of scientific explorers, such as Dalton, Joule, Maxwell, Helmholtz, Herz, Kelvin, and Rayleigh, aided by vast strides made in mechanical skill. But what will our successors be discussing sixty years hence ? How little do we yet know of the vibrations which communicate light and heat ! Far as we have advanced in the application of electricity to the uses of life, we know but little even yet of its real nature. We are only on the threshold of the knowledge of molecular action, or of the constitution of the all-pervading æther. Newton, at the end of the seventeenth century, in his preface to the 'Principia,' says : 'I have deduced the motions of the planets by mathematical reasoning from forces ; and I would that we could derive the other phenomena of Nature from mechanical principles by the same mode of reasoning. For many things move me, so that I somewhat suspect that all such may depend on certain forces by which the particles of bodies, through causes not yet known, are either urged towards each other according to regular figures, or are repelled and recede from each other ; and these forces being unknown, philosophers have hitherto made their attempts on Nature in vain.'

In 1848 Faraday remarked : 'How rapidly the knowledge of molecular forces grows upon us, and how strikingly every investigation tends to develop more and more their importance !

'A few years ago magnetism was an occult force, affecting only a few bodies ; now it is found to influence all bodies, and to possess the most intimate relation with electricity, heat, chemical action, light, crystallisation ; and through it the forces concerned in cohesion. We may feel encouraged to continuous labours, hoping to bring it into a bond of union with gravity itself.'

But it is only within the last few years that we have begun to realise that electricity is closely connected with the vibrations which cause heat and light, and which seem to pervade all space—vibrations which may be termed the voice of the Creator calling to each atom and to each cell of protoplasm to fall into its ordained position, each, as it were, a musical note in the harmonious symphony which we call the universe.

Meteorology.

At the first meeting, in 1831, Professor James D. Forbes was requested to draw up a report on the State of Meteorological Science, on the ground that this science is more in want than any other of that systematic direction which it is one great object of the Association to give.

Professor Forbes made his first report in 1832, and a subsequent report in 1840. The systematic records now kept in various parts of the world of barometric pressure, of solar heat, of the temperature and physical conditions of the atmosphere at various altitudes, of the heat of the ground at various depths, of the rainfall, of the prevalence of winds, and the gradual elucidation not only of the laws which regulate the movements of cyclones and storms, but of the influences which are exercised by the sun and by electricity and magnetism, not only upon atmospheric conditions, but upon health and vitality, are gradually approximating meteorology to the position of an exact science.

England took the lead in rainfall observations. Mr. G. J. Symons organised the British Rainfall System in 1860 with 178 observers, a system which until 1876 received the help of the British Association. Now Mr. Symons himself conducts it, assisted by more than 3,000 observers, and these volunteers not only make the observations, but defray the expense of their reduction and publication. In foreign countries this work is done by Government officers at the public cost.

At the present time a very large number of rain gauges are in daily use throughout the world. The British Islands have more than 3,000, and India and the United States have nearly as many; France and Germany are not far behind; Australia probably has more—indeed, one colony alone, New South Wales, has more than 1,100.

The storm warnings now issued under the excellent systematic organisation of the Meteorological Committee may be said to have had their origin in the terrible storm which broke over the Black Sea during the Crimean War, on November 27, 1855. Leverrier traced the progress of that storm, and seeing how its path could have been reported in advance by the electric telegraph, he proposed to establish observing stations which should report to the coasts the probability of the occurrence of a storm. Leverrier communicated with Airy, and the Government authorised Admiral FitzRoy to make tentative arrangements in this country. The idea was also adopted on the Continent, and now there are few civilised countries north or south of the equator without a system of storm warning.¹

¹ It has often been supposed that Leverrier was also the first to issue a daily weather map, but that was not the case, for in the Great Exhibition of 1851 the Electric Telegraph Company sold daily weather maps, copies of which are still in existence, and the data for them were, it is believed, obtained by Mr. James Glaisher, F.R.S., at that time Superintendent of the Meteorological Department at Greenwich.

BIOLOGICAL SCIENCE.

Botany.

The earliest Reports of the Association which bear on the biological sciences were those relating to botany.

In 1831 the controversy was yet unsettled between the advantages of the Linnean, or Artificial system, as contrasted with the Natural system of classification. Histology, morphology, and physiological botany, even if born, were in their early infancy.

Our records show that von Mohl noted cell division in 1835, the presence of chlorophyll corpuscles in 1837; and he first described protoplasm in 1846.

Vast as have been the advances of physiological botany since that time, much of its fundamental principles remain to be worked out, and I trust that the establishment, for the first time, of a permanent Section for botany at the present meeting will lead the Association to take a more prominent part than it has hitherto done in the further development of this branch of biological science.

Animal Physiology.

In 1831 Cuvier, who during the previous generation had, by the collation of facts followed by careful inductive reasoning, established the plan on which each animal is constructed, was approaching the termination of his long and useful life. He died in 1832; but in 1831 Richard Owen was just commencing his anatomical investigations and his brilliant contributions to palæontology.

The impulse which their labours gave to biological science was reflected in numerous reports and communications, by Owen and others, throughout the early decades of the British Association, until Darwin propounded a theory of evolution which commanded the general assent of the scientific world. For this theory was not absolutely new. But just as Cuvier had shown that each bone in the fabric of an animal affords a clue to the shape and structure of the animal, so Darwin brought harmony into scattered facts, and led us to perceive that the moulding hand of the Creator may have evolved the complicated structures of the organic world from one or more primeval cells.

Richard Owen did not accept Darwin's theory of evolution, and a large section of the public contested it. I well remember the storm it produced—a storm of praise by my geological colleagues, who accepted the result of investigated facts; a storm of indignation such as that which would have burned Galileo at the stake from those who were not yet prepared to question the old authorities; but they diminish daily.

We are, however, as yet only on the threshold of the doctrine of evolution. Does not each fresh investigation, even into the embryonic stage of the simpler forms of life, suggest fresh problems?

Anthropology.

The impulse given by Darwin has been fruitful in leading others to consider whether the same principle of evolution may not have governed the moral as well as the material progress of the human race. Mr. Kidd tells us that nature as interpreted by the struggle for life contains no sanction for the moral progress of the individual, and points out that if each of us were allowed by the conditions of life to follow his own inclination the average of each generation would distinctly deteriorate from that of the preceding one ; but because the law of life is ceaseless and inevitable struggle and competition, ceaseless and inevitable selection and rejection, the result is necessarily ceaseless and inevitable progress. Evolution, as Sir William Flower said, is the message which biology has sent to help us on with some of the problems of human life, and Francis Galton urges that man, the foremost outcome of the awful mystery of evolution, should realise that he has the power of shaping the course of future humanity by using his intelligence to discover and expedite the changes which are necessary to adapt circumstances to man, and man to circumstances.

In considering the evolution of the human race, the science of preventive medicine may afford us some indication of the direction in which to seek for social improvement. One of the early steps towards establishing that science upon a secure basis was taken in 1835 by the British Association, who urged upon the Government the necessity of establishing registers of mortality showing the causes of death 'on one uniform plan in all parts of the King's dominions, as the only means by which general laws touching the influence of causes of disease and death could be satisfactorily deduced.' The general registration of births and deaths was commenced in 1838. But a mere record of death and its proximate cause is insufficient. Preventive medicine requires a knowledge of the details of the previous conditions of life and of occupation. Moreover, death is not our only or most dangerous enemy, and the main object of preventive medicine is to ward off disease. Disease of body lowers our useful energy. Disease of body or of mind may stamp its curse on succeeding generations.

The anthropometric laboratory affords to the student of anthropology a means of analysing the causes of weakness, not only in bodily, but also in mental life.

Mental actions are indicated by movements and their results. Such signs are capable of record, and modern physiology has shown that bodily movements correspond to action in nerve-centres, as surely as the motions of the telegraph-indicator express the movements of the operator's hands in the distant office.

Thus there is a relation between a defective status in brain power and defects in the proportioning of the body. Defects in physiognomical details, too finely graded to be measured with instruments, may be appreciated with accuracy by the senses of the observer ; and the records

show that these defects are, in a large degree, associated with a brain status lower than the average in mental power.

A report presented by one of your committees gives the results of observations made on 100,000 school-children examined individually in order to determine their mental and physical condition for the purpose of classification. This shows that about 16 per 1,000 of the elementary school population appear to be so far defective in their bodily or brain condition as to need special training to enable them to undertake the duties of life, and to keep them from pauperism or crime.

Many of our feeble-minded children, and much disease and vice, are the outcome of inherited proclivities. Francis Galton has shown us that types of criminals which have been bred true to their kind are one of the saddest disfigurements of modern civilisation ; and he says that few deserve better of their country than those who determine to lead celibate lives through a reasonable conviction that their issue would probably be less fitted than the generality to play their part as citizens.

These considerations point to the importance of preventing those suffering from transmissible disease, or the criminal, or the lunatic, from adding fresh sufferers to the teeming misery in our large towns. And in any case, knowing as we do the influence of environment on the development of individuals, they point to the necessity of removing those who are born with feeble minds, or under conditions of moral danger, from surrounding deteriorating influences.

These are problems which materially affect the progress of the human race, and we may feel sure that, as we gradually approach their solution, we shall more certainly realise that the theory of evolution, which the genius of Darwin impressed on this century, is but the first step on a biological ladder which may possibly eventually lead us to understand how in the drama of creation man has been evolved as the highest work of the Creator.

Bacteriology.

The sciences of medicine and surgery were largely represented in the earlier meetings of the Association, before the creation of the British Medical Association afforded a field for their more intimate discussion. The close connection between the different branches of science is causing a revival in our proceedings of discussions on some of the highest medical problems, especially those relating to the spread of infectious and epidemic disease.

It is interesting to contrast the opinion prevalent at the foundation of the Association with the present position of the question.

A report to the Association in 1834, by Professor Henry, on contagion, says :—

‘The notion that contagious emanations are at all connected with the diffusion of animalculæ through the atmosphere is at variance with all that is known of the diffusion of volatile contagion.’

Whilst it had long been known that filthy conditions in air, earth

and water fostered fever, cholera, and many other forms of disease, and that the disease ceased to spread on the removal of these conditions, yet the reason for their propagation or diminution remained under a veil.

Leeuwenhoek in 1680 described the yeast-cells, but Schwann in 1837 first showed clearly that fermentation was due to the activity of the yeast-cells; and, although vague ideas of fermentation had been current during the past century, he laid the foundation of our exact knowledge of the nature of the action of ferments, both organised and unorganised. It was not until 1860, after the prize of the Academy of Sciences had been awarded to Pasteur for his essay against the theory of spontaneous generation, that his investigations into the action of ferments¹ enabled him to show that the effects of the yeast-cell are indissolubly bound up with the activities of the cell as a living organism, and that certain diseases, at least, are due to the action of ferments in the living being. In 1865 he showed that the disease of silkworms, which was then undermining the silk industry in France, could be successfully combated. His further researches into anthrax, fowl cholera, swine fever, rabies, and other diseases proved the theory that those diseases are connected in some way with the introduction of a microbe into the body of an animal; that the virulence of the poison can be diminished by cultivating the microbes in an appropriate manner; and that when the virulence has been thus diminished their inoculation will afford a protection against the disease.

Meanwhile it had often been observed in hospital practice that a patient with a simple-fractured limb was easily cured, whilst a patient with a compound fracture often died from the wound. Lister was thence led, in 1865, to adopt his antiseptic treatment, by which the wound is protected from hostile microbes.

These investigations, followed by the discovery of the existence of a multitude of micro-organisms and the recognition of some of them—such as the bacillus of tubercle and the comma bacillus of cholera—as essential factors of disease; and by the elaboration by Koch and others of methods by which the several organisms might be isolated, cultivated, and their histories studied, have gradually built up the science of bacteriology. Amongst later developments are the discovery of various so-called antitoxins, such as those of diphtheria and tetanus, and the utilisation of these for the cure of disease. Lister's treatment formed a landmark in the science of surgery, and enabled our surgeons to perform operations never before dreamed of; whilst later discoveries are tending to place the practice of medicine on a firm scientific basis. And the science of bacteriology is leading us to recur to stringent rules for the

¹ In speaking of ferments one must bear in mind that there are two classes of ferments: one, living beings, such as yeast—'organised' ferments, as they are sometimes called—the other the products of living beings themselves, such as pepsin, &c.—'unorganised' ferments. Pasteur worked with the former, very little with the latter.

isolation of infectious disease, and to the disinfection (by superheated steam) of materials which have been in contact with the sufferer.

These microbes, whether friendly or hostile, are all capable of multiplying at an enormous rate under favourable conditions. They are found in the air, in water, in the soil ; but, fortunately, the presence of one species appears to be detrimental to other species, and sunshine, or even light from the sky, is prejudicial to most of them. Our bodies, when in health, appear to be furnished with special means of resisting attacks, and, so far as regards their influence in causing disease, the success of the attack of a pathogenic organism upon an individual depends, as a rule, in part at least, upon the power of resistance of the individual.

But notwithstanding our knowledge of the danger arising from a state of low health in individuals, and of the universal prevalence of these micro-organisms, how careless we are in guarding the health conditions of everyday life ! We have ascertained that pathogenic organisms pervade the air. Why, therefore, do we allow our meat, our fish, our vegetables, our easily contaminated milk, to be exposed to their inroads, often in the foulest localities ? We have ascertained that they pervade the water we drink, yet we allow foul water from our dwellings, our pigsties, our farmyards, to pass into ditches without previous clarification, whence it flows into our streams and pollutes our rivers. We know the conditions of occupation which foster ill-health. Why, whilst we remove outside sources of impure air, do we permit the occupation of foul and unhealthy dwellings ?

The study of bacteriology has shown us that although some of these organisms may be the accompaniments of disease, yet we owe it to the operation of others that the refuse caused by the cessation of animal and vegetable life is reconverted into food for fresh generations of plants and animals.

These considerations have formed a point of meeting where the biologist, the chemist, the physicist, and the statistician unite with the sanitary engineer in the application of the science of preventive medicine.

ENGINEERING.

Sewage Purification.

The early reports to the Association show that the laws of hydrostatics, hydrodynamics, and hydraulics necessary to the supply and removal of water through pipes and conduits had long been investigated by the mathematician. But the modern sanitary engineer has been driven by the needs of an increasing population to call in the chemist and the biologist to help him to provide pure water and pure air.

The purification and the utilisation of sewage occupied the attention of the British Association as early as 1864, and between 1869 and 1876 a committee of the Association made a series of valuable reports on the subject. The direct application of sewage to land, though effective as a

means of purification, entailed difficulties in thickly settled districts, owing to the extent of land required.

The chemical treatment of sewage produced an effluent harmless only after having been passed over land, or if turned into a large and rapid stream, or into a tidal estuary; and it left behind a large amount of sludge to be dealt with.

Hence it was long contended that the simplest plan in favourable localities was to turn the sewage into the sea, and that the consequent loss to the land of the manurial value in the sewage would be recouped by the increase in fish-life.

It was not till the chemist called to his aid the biologist, and came to the help of the engineer, that a scientific system of sewage purification was evolved.

Dr. Frankland many years ago suggested the intermittent filtration of sewage; and Mr. Baldwin Latham was one of the first engineers to adopt it. But the valuable experiments made in recent years by the State Board of Health in Massachusetts have more clearly explained to us how by this system we may utilise micro-organisms to convert organic impurity in sewage into food fitted for higher forms of life.

To effect this we require, in the first place, a filter about five feet thick of sand and gravel, or, indeed, of any material which affords numerous surfaces or open pores. Secondly, that after a volume of sewage has passed through the filter, an interval of time be allowed, in which the air necessary to support the life of the micro-organisms is enabled to enter the pores of the filter. Thus this system is dependent upon oxygen and time. Under such conditions the organisms necessary for purification are sure to establish themselves in the filter before it has been long in use. Temperature is a secondary consideration.

Imperfect purification can invariably be traced either to a lack of oxygen in the pores of the filter, or to the sewage passing through so quickly that there is not sufficient time for the necessary processes to take place. And the power of any material to purify either sewage or water depends almost entirely upon its ability to hold a sufficient proportion of either sewage or water in contact with a proper amount of air.

Smoke Abatement.

Whilst the sanitary engineer has done much to improve the surface conditions of our towns, to furnish clean water, and to remove our sewage, he has as yet done little to purify town air. Fog is caused by the floating particles of matter in the air becoming weighted with aqueous vapour; some particles, such as salts of ammonia or chloride of sodium, have a greater affinity for moisture than others. You will suffer from fog so long as you keep refuse stored in your towns to furnish ammonia, or so long as you allow your street surfaces to supply dust, of which much consists of powdered horse manure, or so long as you send the products of

combustion into the atmosphere. Therefore, when you have adopted mechanical traction for your vehicles in towns you may largely reduce one cause of fog. And if you diminish your black smoke, you will diminish black fogs.

In manufactories you may prevent smoke either by care in firing, by using smokeless coal, or by washing the soot out of the products of consumption in its passage along the flue leading to the main chimney-shaft.

The black smoke from your kitchen may be avoided by the use of coke or of gas. But so long as we retain the hygienic arrangement of the open fire in our living-rooms I despair of finding a fireplace, however well constructed, which will not be used in such a manner as to cause smoke, unless, indeed, the chimneys were reversed and the fumes drawn into some central shaft, where they might be washed before being passed into the atmosphere.

Electricity as a warming and cooking agent would be convenient, cleanly, and economical when generated by water power, or possibly wind power, but it is at present too dear when it has to be generated by means of coal. I can conceive, however, that our descendants may learn so to utilise electricity that they in some future century may be enabled by its means to avoid the smoke in their towns.

Mechanical Engineering.

In other branches of civil and mechanical engineering, the reports in 1831 and 1832 on the state of this science show that the theoretical and practical knowledge of the strength of timber had obtained considerable development. But in 1830, before the introduction of railways, cast iron had been sparingly used in arched bridges for spans of from 160 to 200 feet, and wrought iron had only been applied to large-span iron bridges on the suspension principle, the most notable instance of which was the Menai Suspension Bridge, by Telford. Indeed, whilst the strength of timber had been patiently investigated by engineers, the best form for the use of iron girders and struts was only beginning to attract attention, and the earlier volumes of our Proceedings contained numerous records of the researches of Eaton Hodgkinson, Barlow, Rennie, and others. It was not until twenty years later that Robert Stephenson and William Fairbairn erected the tubular bridge at Menai, followed by the more scientific bridge erected by Brunel at Saltash. These have now been entirely eclipsed by the skill with which the estuary of the Forth has been bridged with a span of 1,700 feet by Sir John Fowler and Sir Benjamin Baker.

The development of the iron industry is due to the association of the chemist with the engineer. The introduction of the hot blast by Neilson, in 1829, in the manufacture of cast iron had effected a large saving of fuel. But the chemical conditions which affect the strength and other qualities of iron, and its combinations with carbon, silicon, phosphorus, and other substances, had at that time scarcely been investigated.

In 1856 Bessemer brought before the British Association at Cheltenham his brilliant discovery for making steel direct from the blast furnace, by which he dispensed with the laborious process of first removing the carbon from pig-iron by puddling, and then adding by cementation the required proportion of carbon to make steel. This discovery, followed by Siemens's regenerative furnace, by Whitworth's compressed steel, and by the use of alloys and by other improvements too numerous to mention here, have revolutionised the conditions under which metals are applied to engineering purposes.

Indeed, few questions are of greater interest, or possess more industrial importance, than those connected with metallic alloys. This is especially true of those alloys which contain the rarer metals; and the extraordinary effects of small quantities of chromium, nickel, tungsten and titanium on certain varieties of steel have exerted profound influence on the manufacture of projectiles and on the construction of our armoured ships.

Of late years, investigations on the properties and structure of alloys have been numerous, and among the more noteworthy researches may be mentioned those of Dewar and Fleming on the distinctive behaviour, as regards the thermo-electric powers and electrical resistance, of metals and alloys at the very low temperatures which may be obtained by the use of liquid air.

Professor Roberts-Austen, on the other hand, has carefully studied the behaviour of alloys at very high temperatures, and by employing his delicate pyrometer has obtained photographic curves which afford additional evidence as to the existence of allotropic modifications of metals, and which have materially strengthened the view that alloys are closely analogous to saline solutions. In this connection it may be stated that the very accurate work of Heycock and Neville on the lowering of the solidifying points of molten metals, which is caused by the presence of other metals, affords a valuable contribution to our knowledge.

Professor Roberts-Austen has, moreover, shown that the effect of any one constituent of an alloy upon the properties of the principal metal has a direct relation to the atomic volumes, and that it is consequently possible to foretell, in a great measure, the effect of any given combination.

A new branch of investigation, which deals with the micro-structure of metals and alloys, is rapidly assuming much importance. It was instituted by Sorby in a communication which he made to the British Association in 1864, and its development is due to many patient workers, among whom M. Osmond occupies a prominent place.

Metallurgical science has brought aluminium into use by cheapening the process of its extraction; and if by means of the wasted forces in our rivers, or possibly of the wind, the extraction be still further cheapened by the aid of electricity, we may not only utilise the metal or its alloys in increasing the spans of our bridges, and in affording strength and lightness in the construction of our ships, but we may hope to obtain a material which may render practicable the dreams of Icarus and of Maxim, and for purposes of rapid transit enable us to navigate the air.

Long before 1831 the steam-engine had been largely used on rivers and lakes, and for short sea passages, although the first Atlantic steam-service was not established till 1838.

As early as 1820 the steam-engine had been applied by Gurney, Hancock, and others to road traction. The absurd impediments placed in their way by road trustees, which, indeed, are still enforced, checked any progress. But the question of mechanical traction on ordinary roads was practically shelved in 1830, at the time of the formation of the British Association, when the locomotive engine was combined with a tubular boiler and an iron road on the Liverpool and Manchester Railway.

Great, however, as was the advance made by the locomotive engine of Robert Stephenson, these earlier engines were only toys compared with the compound engines of to-day which are used for railways, for ships, or for the manufacture of electricity. Indeed, it may be said that the study of the laws of heat, which have led to the introduction of various forms of motive power, are gradually revolutionising all our habits of life.

The improvements in the production of iron, combined with the developed steam-engine, have completely altered the conditions of our commercial intercourse on land ; whilst the changes caused by the effects of these improvements in shipbuilding, and on the ocean carrying trade, have been, if anything, still more marked.

At the foundation of the Association all ocean ships were built by hand, of wood, propelled by sails and manœuvred by manual labour ; the material limited their length, which did not often exceed 100 feet, and the number of English ships of over 500 tons burden was comparatively small.

In the modern ships steam power takes the place of manual labour. It rolls the plates of which the ship is constructed, bends them to the required shape, cuts, drills and rivets them in their place. It weighs the anchor ; it propels the ship in spite of winds or currents ; it steers, ventilates, and lights the ship when on the ocean. It takes the cargo on board and discharges it on arrival.

The use of iron favours the construction of ships of a large size, of forms which afford small resistance to the water, and with compartments which make the ships practically unsinkable in heavy seas, or by collision. Their size, the economy with which they are propelled, and the certainty of their arrival, cheapens the cost of transport.

The steam-engine, by compressing air, gives us control over the temperature of cool chambers. In these not only fresh meat, but the delicate produce of the Antipodes, is brought across the ocean to our doors without deterioration.

Whilst railways have done much to alter the social conditions of each individual nation, the application of iron and steam to our ships is revolutionising the international commercial conditions of the world ; and it is gradually changing the course of our agriculture, as well as of our domestic life.

But great as have been the developments of science in promoting the commerce of the world, science is asserting its supremacy even to a greater extent in every department of war. And perhaps this application of science affords at a glance, better than almost any other, a convenient illustration of the assistance which the chemical, physical, and electrical sciences are affording to the engineer.

The reception of warlike stores is not now left to the uncertain judgment of 'practical men,' but is confided to officers who have received a special training in chemical analysis, and in the application of physical and electrical science to the tests by which the qualities of explosives, of guns, and of projectiles can be ascertained.

For instance, take explosives. Till quite recently black and brown powders alone were used, the former as old as civilisation, the latter but a small modern improvement adapted to the increased size of guns. But now the whole family of nitro-explosives are rapidly superseding the old powder. These are the direct outcome of chemical knowledge; they are not mere chance inventions, for every improvement is based on chemical theories, and not on random experiment.

The construction of guns is no longer a haphazard operation. In spite of the enormous forces to be controlled and the sudden violence of their action, the researches of the mathematician have enabled the just proportions to be determined with accuracy; the labours of the physicist have revealed the internal conditions of the materials employed, and the best means of their favourable employment. Take, for example, Longridge's coiled-wire system, in which each successive layer of which the gun is formed receives the exact proportion of tension which enables all the layers to act in unison. The chemist has rendered it clear that even the smallest quantities of certain ingredients are of supreme importance in affecting the tenacity and trustworthiness of the materials.

The treatment of steel to adapt it to the vast range of duties it has to perform is thus the outcome of patient research. And the use of the metals—manganese, chromium, nickel, molybdenum—as alloys with iron has resulted in the production of steels possessing varied and extraordinary properties. The steel required to resist the conjugate stresses developed, lightning fashion, in a gun necessitates qualities that would not be suitable in the projectile which that gun hurls with a velocity of some 2,500 feet per second against the armoured side of a ship. The armour, again, has to combine extreme superficial hardness with great toughness, and during the last few years these qualities are sought to be attained by the application of the cementation process for adding carbon to one face of the plate, and hardening that face alone by rapid refrigeration.

The introduction of quick-firing guns from '303 (*i.e.* about one-third) of an inch to 6-inch calibre has rendered necessary the production of metal cartridge-cases of complex forms drawn cold out of solid blocks or plate of the material; this again has taxed the ingenuity of the mechanic in the device of machinery, and of the metallurgist in producing a metal possessed

of the necessary ductility and toughness. The cases have to stand a pressure at the moment of firing of as much as twenty-five tons to the square inch—a pressure which exceeds the ordinary elastic limits of the steel of which the gun itself is composed.

There is nothing more wonderful in practical mechanics than the closing of the breech openings of guns, for not only must they be gas-tight at these tremendous pressures, but the mechanism must be such that one man by a single continuous movement shall be able to open or close the breech of the largest gun in some ten or fifteen seconds.

The perfect knowledge of the recoil of guns has enabled the reaction of the discharge to be utilised in compressing air or springs by which guns can be raised from concealed positions in order to deliver their fire, and then made to disappear again for loading ; or the same force has been used to run up the guns automatically immediately after firing, or, as in the case of the Maxim gun, to deliver in the same way a continuous stream of bullets at the rate of ten in one second.

In the manufacture of shot and shell cast iron has been almost superseded by cast and wrought steel, though the hardened Palliser projectiles still hold their place. The forged-steel projectiles are produced by methods very similar to those used in the manufacture of metal cartridge-cases, though the process is carried on at a red heat and by machines much more powerful.

In every department concerned in the production of warlike stores electricity is playing a more and more important part. It has enabled the passage of a shot to be followed from its seat in the gun to its destination.

In the gun, by means of electrical contacts arranged in the bore, a time-curve of the passage of the shot can be determined.

From this the mathematician constructs the velocity-curve, and from this, again, the pressures producing the velocity are estimated, and used to check the same indications obtained by other means. The velocity of the shot after it has left the gun is easily ascertained by the Boulangé apparatus.

Electricity and photography have been laid under contribution for obtaining records of the flight of projectiles and the effects of explosions at the moment of their occurrence. Many of you will recollect Mr. Vernon Boys' marvellous photographs showing the progress of the shot driving before it waves of air in its course.

Electricity and photography also record the properties of metals and their alloys as determined by curves of cooling.

The readiness with which electrical energy can be converted into heat or light has been taken advantage of for the firing of guns, which in their turn can, by the same agency, be laid on the object by means of range-finders placed at a distance and in advantageous and safe positions ; while the electric light is utilised to illumine the sights at night, as well as to search out the objects of attack.

The compact nature of the glow-lamp, the brightness of the light, the circumstance that the light is not due to combustion, and therefore independent of air, facilitates the examination of the bore of guns, the insides of shells, and other similar uses—just as it is used by a doctor to examine the throat of a patient.

INFLUENCE OF INTERCOMMUNICATION AFFORDED BY BRITISH ASSOCIATION ON SCIENCE PROGRESS.

The advances in engineering which have produced the steam-engine, the railway, the telegraph, as well as our engines of war, may be said to be the result of commercial enterprise rendered possible only by the advances which have taken place in the several branches of science since 1831. Having regard to the intimate relations which the several sciences bear to each other, it is abundantly clear that much of this progress could not have taken place in the past, nor could further progress take place in the future, without intercommunication between the students of different branches of science.

The founders of the British Association based its claims to utility upon the power it afforded for this intercommunication. Mr. Vernon Harcourt (the uncle of your present General Secretary), in the address he delivered in 1832, said: ‘How feeble is man for any purpose when he stands alone—how strong when united with other men!’

‘It may be true that the greatest philosophical works have been achieved in privacy, but it is no less true that these works would never have been accomplished had the authors not mingled with men of corresponding pursuits, and from the commerce of ideas often gathered germs of apparently insulated discoveries, and without such material aid would seldom have carried their investigations to a valuable conclusion.’

I claim for the British Association that it has fulfilled the objects of its founders, that it has had a large share in promoting intercommunication and combination.

Our meetings have been successful because they have maintained the true principles of scientific investigation. We have been able to secure the continued presence and concurrence of the master-spirits of science. They have been willing to sacrifice their leisure, and to promote the welfare of the Association, because the meetings have afforded them the means of advancing the sciences to which they are attached.

The Association has, moreover, justified the views of its founders in promoting intercourse between the pursuers of science, both at home and abroad, in a manner which is afforded by no other agency.

The weekly and sessional reunions of the Royal Society, and the annual *soirées* of other scientific societies, promote this intercourse to some extent, but the British Association presents to the young student during its week of meetings easy and continuous social opportunities for

making the acquaintance of leaders in science, and thereby obtaining their directing influence.

It thus encourages, in the first place, opportunities of combination, but, what is equally important, it gives at the same time material assistance to the investigators whom it thus brings together.

The reports on the state of science at the present time, as they appear in the last volume of our Proceedings, occupy the same important position, as records of science progress, as that occupied by those Reports in our earlier years. We exhibit no symptom of decay.

SCIENCE IN GERMANY FOSTERED BY THE STATE AND MUNICIPALITIES.

Our neighbours and rivals rely largely upon the guidance of the State for the promotion of both science teaching and of research. In Germany the foundations of technical and industrial training are laid in the *Realschulen*, and supplemented by the Higher Technical Schools. In Berlin that splendid institution, the Royal Technical High School, casts into the shade the facilities for education in the various Polytechnics which we are now establishing in London. Moreover, it assists the practical workman by a branch department, which is available to the public for testing building materials, metals, paper, oil, and other matters. The standards of all weights and measures used in trade can be purchased from or tested by the Government Department for Weights and Measures.

For developing pure scientific research and for promoting new applications of science to industrial purposes the German Government, at the instance of von Helmholtz, and aided by the munificence of Werner von Siemens, created the *Physikalische Technische Reichsanstalt* at Charlottenburg.

This establishment consists of two divisions. The first is charged with pure research, and is at the present time engaged in various thermal, optical, and electrical and other physical investigations. The second branch is employed in operations of delicate standardising to assist the wants of research students—for instance, dilatation, electrical resistances, electric and other forms of light, pressure gauges, recording instruments, thermometers, pyrometers, tuning forks, glass, oil-testing apparatus, viscosity of glycerine, &c.

Dr. Kohlrausch succeeded Helmholtz as president, and takes charge of the first division. Professor Hagen, the director under him, has charge of the second division. A professor is in charge of each of the several sub-departments. Under these are various subordinate posts, held by younger men, selected for previous valuable work, and usually for a limited time.

The general supervision is under a Council consisting of a president, who is a Privy Councillor, and twenty-four members, including the president and director of the *Reichsanstalt*; of the other members, about ten are professors or heads of physical and astronomical observatories

connected with the principal universities in Germany. Three are selected from leading firms in Germany representing mechanical, optical, and electric science, and the remainder are principal scientific officials connected with the Departments of War and Marine, the Royal Observatory at Potsdam, and the Royal Commission for Weights and Measures.

This Council meets in the winter, for such time as may be necessary, for examining the research work done in the first division during the previous year, and for laying down the scheme for research for the ensuing year ; as well as for suggesting any requisite improvements in the second division. As a consequence of the position which science occupies in connection with the State in Continental countries, the services of those who have distinguished themselves either in the advancement or in the application of science are recognised by the award of honours ; and thus the feeling for science is encouraged throughout the nation.

ASSISTANCE TO SCIENTIFIC RESEARCH IN GREAT BRITAIN.

Great Britain maintained for a long time a leading position among the nations of the world by virtue of the excellence and accuracy of its workmanship, the result of individual energy ; but the progress of mechanical science has made accuracy of workmanship the common property of all nations of the world. Our records show that hitherto, in its efforts to maintain its position by the application of science and the prosecution of research, England has made marvellous advances by means of voluntary effort, illustrated by the splendid munificence of such men as Gassiot, Joseph Whitworth, James Mason, and Ludwig Mond ; and, whilst the increasing field of scientific research compels us occasionally to seek for Government assistance, it would be unfortunate if by any change voluntary effort were fettered by State control.

The following are the principal voluntary agencies which help forward scientific research in this country :—The Donation Fund of the Royal Society, derived from its surplus income. The British Association has contributed 60,000*l.* to aid research since its formation. The Royal Institution, founded in the last century, by Count Rumford, for the promotion of research, has assisted the investigations of Davy, of Young, of Faraday, of Frankland, of Tyndall, of Dewar, and of Rayleigh. The City Companies assist scientific research and foster scientific education both by direct contributions and through the City and Guilds Institute. The Commissioners of the Exhibition of 1851 devote 6,000*l.* annually to science research scholarships, to enable students who have passed through a college curriculum and have given evidence of capacity for original research to continue the prosecution of science, with a view to its advance or to its application to the industries of the country. Several scientific societies, as, for instance, the Geographical Society and the Mechanical Engineers, have promoted direct research, each in their own branch of science, out of their surplus income ; and every scientific society largely assists research by the publication, not only of its own proceedings, but

often of the work going on abroad in the branch of science which it represents.

The growing abundance of matter year by year increases the burden thus thrown on their finances, and the Treasury has recently granted to the Royal Society 1,000*l.* a year, to be spent in aid of the publication of scientific papers not necessarily limited to those of that Society.

The Royal Society has long felt the importance to scientific research of a catalogue of all papers and publications relating to pure and applied science, arranged systematically both as to authors' names and as to subject treated, and the Society has been engaged for some time upon a catalogue of that nature. But the daily increasing magnitude of these publications, coupled with the necessity of issuing the catalogue with adequate promptitude, and at appropriate intervals, renders it a task which could only be performed under International co-operation. The officers of the Royal Society have therefore appealed to the Government to urge Foreign Governments to send delegates to a Conference to be held next July to discuss the desirability and the scope of such a catalogue, and the possibility of preparing it.

The universities and colleges distributed over the country, besides their function of teaching, are large promoters of research, and their voluntary exertions are aided in some cases by contributions from Parliament in alleviation of their expenses.

Certain executive departments of the Government carry on research for their own purposes, which in that respect may be classed as voluntary. The Admiralty maintains the Greenwich Observatory, the Hydrographical Department, and various experimental services; and the War Office maintains its numerous scientific departments. The Treasury maintains a valuable chemical laboratory for Inland Revenue, Customs, and agricultural purposes. The Science and Art Department maintains the Royal College of Science, for the education of teachers and students from elementary schools. It allows the scientific apparatus in the national museum to be used for research purposes by the professors. The Solar Physics Committee, which has carried on numerous researches in solar physics, was appointed by and is responsible to this Department. The Department also administers the Sir Joseph Whitworth engineering research scholarships. Other scientific departments of the Government are aids to research, as, for instance, the Ordnance and the Geological Surveys, the Royal Mint, the Natural History Museum, Kew Gardens, and other lesser establishments in Scotland and Ireland; to which may be added, to some extent, the Standards Department of the Board of Trade, as well as municipal museums, which are gradually spreading over the country.

For direct assistance to voluntary effort the Treasury contributes 4,000*l.* a year to the Royal Society for the promotion of research, which is administered under a board whose members represent all branches of Science. The Treasury, moreover, contributes to marine biological observatories, and in recent years has defrayed the cost of various expedi-

tions for biological and astronomical research, which in the case of the 'Challenger' expedition involved very large sums of money.

In addition to these direct aids to science, Parliament, under the Local Taxation Act, handed over to the County Councils a sum, which amounted in the year 1893 to 615,000*l.*, to be expended on technical education. In many country districts, so far as the advancement of real scientific technical progress in the nation is concerned, much of this money has been wasted for want of knowledge. And whilst it cannot be said that the Government or Parliament have been indifferent to the promotion of scientific education and research, it is a source of regret that the Government did not devote some small portion of this magnificent gift to affording an object-lesson to County Councils in the application of science to technical instruction, which would have suggested the principles which would most usefully guide them in the expenditure of this public money.

Government assistance to science has been based mainly on the principle of helping voluntary effort. The Kew Observatory was initiated as a scientific observatory by the British Association. It is now supported by the Gassiot trust fund, and managed by the Kew Observatory Committee of the Royal Society. Observations on magnetism, on meteorology, and the record of sun-spots, as well as experiments upon new instruments for assisting meteorological, thermometrical, and photographic purposes, are being carried on there. The Committee has also arranged for the verification of scientific measuring instruments, the rating of chronometers, the testing of lenses and of other scientific apparatus. This institution carries on to a limited extent some small portion of the class of work done in Germany by that magnificent institution, the Reichsanstalt at Charlottenburg, but its development is fettered by want of funds. British students of science are compelled to resort to Berlin and Paris when they require to compare their more delicate instruments and apparatus with recognised standards. There could scarcely be a more advantageous addition to the assistance which Government now gives to science than for it to allot a substantial annual sum to the extension of the Kew Observatory, in order to develop it on the model of the Reichsanstalt. It might advantageously retain its connection with the Royal Society, under a Committee of Management representative of the various branches of science concerned, and of all parts of Great Britain.

CONCLUSION.

The various agencies for scientific education have produced numerous students admirably qualified to pursue research; and at the same time almost every field of industry presents openings for improvement through the development of scientific methods. For instance, agricultural operations alone offer openings for research to the biologist, the chemist, the physicist, the geologist, the engineer, which have hitherto been largely

overlooked. If students do not easily find employment, it is chiefly attributable to a want of appreciation for science in the nation at large.

This want of appreciation appears to arise from the fact that those who nearly half a century ago directed the movement of national education were trained in early life in the universities, in which the value of scientific methods was not at that time fully recognised. Hence our elementary, and even our secondary and great public schools, neglected for a long time to encourage the spirit of investigation which develops originality. This defect is diminishing daily.

There is, however, a more intangible cause which may have had influence on the want of appreciation of science by the nation. The Government, which largely profits by science, aids it with money, but it has done very little to develop the national appreciation for science by recognising that its leaders are worthy of honours conferred by the State. Science is not fashionable, and science students—upon whose efforts our progress as a nation so largely depends—have not received the same measure of recognition which the State awards to services rendered by its own officials, by politicians, and by the Army and by the Navy, whose success in future wars will largely depend on the effective applications of science.

The Reports of the British Association afford a complete chronicle of the gradual growth of scientific knowledge since 1831. They show that the Association has fulfilled the objects of its founders in promoting and disseminating a knowledge of science throughout the nation.

The growing connection between the sciences places our annual meeting in the position of an arena where representatives of the different sciences have the opportunity of criticising new discoveries and testing the value of fresh proposals, and the Presidential and Sectional Addresses operate as an annual stock-taking of progress in the several branches of science represented in the Sections. Every year the field of usefulness of the Association is widening. For, whether with the geologist we seek to write the history of the crust of the earth, or with the biologist to trace out the evolution of its inhabitants, or whether with the astronomer, the chemist, and the physicist we endeavour to unravel the constitution of the sun and the planets or the genesis of the nebulae and stars which make up the universe, on every side we find ourselves surrounded by mysteries which await solution. We are only at the beginning of work.

I have, therefore, full confidence that the future records of the British Association will chronicle a still greater progress than that already achieved, and that the British nation will maintain its leading position amongst the nations of the world, if it will energetically continue its voluntary efforts to promote research, supplemented by that additional help from the Government which ought never to be withheld when a clear case of scientific utility has been established.

British Association for the Advancement of Science.

LIVERPOOL, 1896.

ADDRESS

BY

SIR JOSEPH LISTER, BART., D.C.L., LL.D., P.R.S.,
PRESIDENT.

My Lord Mayor, my Lords, Ladies, and Gentlemen, I have first to express my deep sense of gratitude for the great honour conferred upon me by my election to the high office which I occupy to-day. It came upon me as a great surprise. The engrossing claims of surgery have prevented me for many years from attending the meetings of the Association, which excludes from her sections medicine in all its branches. This severance of the art of healing from the work of the Association was right and indeed inevitable. Not that medicine has little in common with science. The surgeon never performs an operation without the aid of anatomy and physiology; and in what is often the most difficult part of his duty, the selection of the right course to follow, he, like the physician, is guided by pathology, the science of the nature of disease, which, though very difficult from the complexity of its subject matter, has made during the last half-century astonishing progress; so that the practice of medicine in every department is becoming more and more based on science as distinguished from empiricism. I propose on the present occasion to bring before you some illustrations of the interdependence of science and the healing art; and the first that I will take is perhaps the most astonishing of all results of purely physical inquiry—the discovery of the Röntgen rays, so called after the man who first clearly revealed them to the world. Mysterious as they still are, there is one of their properties which we can all appreciate—their power of passing through substances opaque to ordinary light. There seems to be no relation whatever between transparency in the common sense of

the term and penetrability to these emanations. The glasses of a pair of spectacles may arrest them while their wooden and leathern case allows them to pass almost unchecked. Yet they produce, whether directly or indirectly, the same effects as light upon a photographic plate. As a general rule the denser any object is the greater obstacle does it oppose to the rays. Hence, as bone is denser than flesh, if the hand or other part of the body is placed above the sensitive film enclosed in a case of wood or other light material at a suitable distance from the source of the rays, while they pass with the utmost facility through the uncovered parts of the lid of the box and powerfully affect the plate beneath, they are arrested to a large extent by the bones, so that the plate is little acted upon in the parts opposite to them, while the portions corresponding to the muscles and other soft parts are influenced in an intermediate degree. Thus a picture is obtained in which the bones stand out in sharp relief among the flesh, and anything abnormal in their shape or position is clearly displayed.

I need hardly point out what important aid this must give to the surgeon. As an instance, I may mention a case which occurred in the practice of Mr. Howard Marsh. He was called to see a severe injury of the elbow, in which the swelling was so great as to make it impossible for him by ordinary means of examination to decide whether he had to deal with a fracture or a dislocation. If it were the latter, a cure would be effected by the exercise of violence which would be not only useless but most injurious if a bone was broken. By the aid of the Röntgen rays a photograph was taken in which the bone of the upper arm was clearly seen displaced forwards on those of the forearm. The diagnosis being thus established, Mr. Marsh proceeded to reduce the dislocation ; and his success was proved by another photograph which showed the bones in their natural relative position.

The common metals, such as lead, iron, and copper, being still denser than the osseous structures, these rays can show a bullet embedded in a bone or a needle lodged about a joint. At the last conversazione of the Royal Society a picture produced by the new photography displayed with perfect distinctness through the bony framework of the chest a halfpenny low down in a boy's gullet. It had been there for six months, causing uneasiness at the pit of the stomach during swallowing ; but whether the coin really remained impacted, or if so, what was its position, was entirely uncertain till the Röntgen rays revealed it. Dr. Macintyre of Glasgow, who was the photographer, informs me that when the presence of the halfpenny had been thus demonstrated, the surgeon in charge of the case made an attempt to extract it, and although this was not successful in its immediate object, it had the effect of dislodging the coin ; for a subsequent photograph by Dr. Macintyre not only showed that it had disappeared from the gullet, but also, thanks to the wonderful penetrating power which the rays had acquired in his hands, proved that it had not

lodged further down in the alimentary passage. The boy has since completely recovered.

The Röntgen rays cause certain chemical compounds to fluoresce, and emit a faint light plainly visible in the dark ; and if they are made to fall upon a translucent screen impregnated with such a salt, it becomes beautifully illuminated. If a part of the human body is interposed between the screen and the source of the rays, the bones and other structures are thrown in shadow upon it, and thus a diagnosis can be made without the delay involved in taking a photograph. It was in fact in this way that Dr. Macintyre first detected the coin in the boy's gullet. Mr. Herbert Jackson, of King's College, London, early distinguished himself in this branch of the subject. There is no reason to suppose that the limits of the capabilities of the rays in this way have yet been reached. By virtue of the greater density of the heart than the adjacent lungs with their contained air, the form and dimensions of that organ in the living body may be displayed on the fluorescent screen, and even its movements have been lately seen by several different observers.

Such important applications of the new rays to medical practice have strongly attracted the interest of the public to them, and I venture to think that they have even served to stimulate the investigations of physicists. The eminent Professor of Physics in the University College of this city (Professor Lodge) was one of the first to make such practical applications, and I was able to show to the Royal Society at a very early period a photograph, which he had the kindness to send me, of a bullet embedded in the hand. His interest in the medical aspect of the subject remains unabated, and at the same time he has been one of the most distinguished investigators of its purely physical side.

There is another way in which the Röntgen rays connect themselves with physiology, and may possibly influence medicine. It is found that if the skin is long exposed to their action it becomes very much irritated, affected with a sort of aggravated sun-burning. This suggests the idea that the transmission of the rays through the human body may be not altogether a matter of indifference to internal organs, but may, by long-continued action, produce, according to the condition of the part concerned, injurious irritation or salutary stimulation.

This is the jubilee of Anæsthesia in surgery. That priceless blessing to mankind came from America. It had, indeed, been foreshadowed in the first year of this century by Sir Humphry Davy, who, having found a toothache from which he was suffering relieved as he inhaled laughing gas (nitrous oxide), threw out the suggestion that it might perhaps be used for preventing pain in surgical operations. But it was not till, on September 30, 1846, Dr. W. T. G. Morton, of Boston, after a series of experiments upon himself and the lower animals, extracted a tooth painlessly from a patient whom he had caused to inhale the vapour of sulphuric ether, that the idea was fully realised. He soon afterwards publicly

exhibited his method at the Massachusetts General Hospital, and after that event the great discovery spread rapidly over the civilised world. I witnessed the first operation in England under ether. It was performed by Robert Liston in University College Hospital, and it was a complete success. Soon afterwards I saw the same great surgeon amputate the thigh as painlessly, with less complicated anæsthetic apparatus, by aid of another agent, chloroform, which was being powerfully advocated as a substitute for ether by Dr. (afterwards Sir James Y.) Simpson, who also had the great merit of showing that confinements could be conducted painlessly, yet safely, under its influence. These two agents still hold the field as general anæsthetics for protracted operations, although the gas originally suggested by Davy, in consequence of its rapid action and other advantages, has taken their place in short operations, such as tooth extraction. In the birthplace of anæsthesia ether has always maintained its ground; but in Europe it was to a large extent displaced by chloroform till recently, when many have returned to ether, under the idea that, though less convenient, it is safer. For my own part, I believe that chloroform, if carefully administered on right principles, is, on the average, the safer agent of the two.

The discovery of anæsthesia inaugurated a new era in surgery. Not only was the pain of operations abolished, but the serious and sometimes mortal shock which they occasioned to the system was averted, while the patient was saved the terrible ordeal of preparing to endure them. At the same time the field of surgery became widely extended, since many procedures in themselves desirable, but before impossible from the protracted agony they would occasion, became matters of routine practice. Nor have I by any means exhausted the list of the benefits conferred by this discovery.

Anæsthesia in surgery has been from first to last a gift of science. Nitrous oxide, sulphuric ether, and chloroform are all artificial products of chemistry, their employment as anæsthetics was the result of scientific investigation, and their administration, far from being, like the giving of a dose of medicine, a matter of rule of thumb, imperatively demands the vigilant exercise of physiological and pathological knowledge.

While rendering such signal service to surgery, anæsthetics have thrown light upon biology generally. It has been found that they exert their soporific influence not only upon vertebrata, but upon animals so remote in structure from man as bees and other insects. Even the functions of vegetables are suspended by their agency. They thus afford strong confirmation of the great generalisation that living matter is of the same essential nature wherever it is met with on this planet, whether in the animal or vegetable kingdom. Anæsthetics have also, in ways to which I need not here refer, powerfully promoted the progress of physiology and pathology.

My next illustration may be taken from the work of Pasteur on fer-

mentation. The prevailing opinion regarding this class of phenomena when they first engaged his attention was that they were occasioned primarily by the oxygen of the air acting upon unstable animal or vegetable products, which, breaking up under its influence, communicated disturbance to other organic materials in their vicinity, and thus led to their decomposition. Cagniard-Latour had indeed shown several years before that yeast consists essentially of the cells of a microscopic fungus which grows as the sweetwort ferments ; and he had attributed the breaking up of the sugar into alcohol and carbonic acid to the growth of the micro-organism. In Germany Schwann, who independently discovered the yeast plant, had published very striking experiments in support of analogous ideas regarding the putrefaction of meat. Such views had also found other advocates, but they had become utterly discredited, largely through the great authority of Liebig, who bitterly opposed them.

Pasteur, having been appointed as a young man Dean of the Faculty of Sciences in the University of Lille, a town where the products of alcoholic fermentation were staple articles of manufacture, determined to study that process thoroughly ; and as a result he became firmly convinced of the correctness of Cagniard-Latour's views regarding it. In the case of other fermentations, however, nothing fairly comparable to the formation of yeast had till then been observed. This was now done by Pasteur for that fermentation in which sugar is resolved into lactic acid. This lactic fermentation was at that time brought about by adding some animal substance, such as fibrin, to a solution of sugar, together with chalk that should combine with the acid as it was formed. Pasteur saw, what had never before been noticed, that a fine grey deposit was formed, differing little in appearance from the decomposing fibrin, but steadily increasing as the fermentation proceeded. Struck by the analogy presented by the increasing deposit to the growth of yeast in sweetwort, he examined it with the microscope, and found it to consist of minute particles of uniform size. Pasteur was not a biologist, but although these particles were of extreme minuteness in comparison with the constituents of the yeast plant, he felt convinced that they were of an analogous nature, the cells of a tiny microscopic fungus. This he regarded as the essential ferment, the fibrin or other so-called ferment serving, as he believed, merely the purpose of supplying to the growing plant certain chemical ingredients not contained in the sugar but essential to its nutrition. And the correctness of this view he confirmed in a very striking manner, by doing away with the fibrin or other animal material altogether, and substituting for it mineral salts containing the requisite chemical elements. A trace of the grey deposit being applied to a solution of sugar containing these salts in addition to the chalk, a brisker lactic fermentation ensued than could be procured in the ordinary way.

I have referred to this research in some detail because it illustrates

Pasteur's acuteness as an observer and his ingenuity in experiment, as well as his almost intuitive perception of truth.

A series of other beautiful investigations followed, clearly proving that all true fermentations, including putrefaction, are caused by the growth of micro-organisms.

It was natural that Pasteur should desire to know how the microbes which he showed to be the essential causes of the various fermentations took their origin. It was at that period a prevalent notion, even among many eminent naturalists, that such humble and minute beings originated *de novo* in decomposing organic substances ; the doctrine of spontaneous generation, which had been chased successively from various positions which it once occupied among creatures visible to the naked eye, having taken its last refuge where the objects of study were of such minuteness that their habits and history were correspondingly difficult to trace. Here again Pasteur at once saw, as if by instinct, on which side the truth lay ; and, perceiving its immense importance, he threw himself with ardour into its demonstration. I may describe briefly one class of experiments which he performed with this object. He charged a series of narrow-necked glass flasks with a decoction of yeast, a liquid peculiarly liable to alteration on exposure to the air. Having boiled the liquid in each flask, to kill any living germs it might contain, he sealed its neck with a blow-pipe during ebullition ; after which, the flask being allowed to cool, the steam within it condensed, leaving a vacuum above the liquid. If, then, the neck of the flask were broken in any locality, the air at that particular place would rush in to fill the vacuum, carrying with it any living microbes that might be floating in it. The neck of the flask having been again sealed, any germs so introduced would in due time manifest their presence by developing in the clear liquid. When any of such a series of flasks were opened and re-sealed in an inhabited room, or under the trees of a forest, multitudes of minute living forms made their appearance in them ; but if this was done in a cellar long unused, where the suspended organisms, like other dust, might be expected to have all fallen to the ground, the decoction remained perfectly clear and unaltered. The oxygen and other gaseous constituents of the atmosphere were thus shown to be of themselves incapable of inducing any organic development in yeast-water.

Such is a sample of the many well-devised experiments by which he carried to most minds the conviction that, as he expressed it, '*la génération spontanée est une chimère*,' and that the humblest and minutest living organisms can only originate by parentage from beings like themselves.

Pasteur pointed out the enormous importance of these humble organisms in the economy of nature. It is by their agency that the dead bodies of plants and animals are resolved into simpler compounds fitted for assimilation by new living forms. Without their aid the world would be, as Pasteur expresses it, *encombré de cadavres*. They are essential not only to our well-being, but to our very existence. Similar microbes must

have discharged the same necessary function of removing refuse and providing food for successive generations of plants and animals during the past periods of the world's history ; and it is interesting to think that organisms as simple as can well be conceived to have existed when life first appeared upon our globe have, in all probability, propagated the same lowly but most useful offspring during the ages of geological time.

Pasteur's labours on fermentation have had a very important influence upon surgery. I have been often asked to speak on my share in this matter before a public audience ; but I have hitherto refused to do so, partly because the details are so entirely technical, but chiefly because I have felt an invincible repugnance to what might seem to savour of self-advertisement. The latter objection now no longer exists, since advancing years have indicated that it is right for me to leave to younger men the practice of my dearly loved profession. And it will perhaps be expected that, if I can make myself intelligible, I should say something upon the subject on the present occasion.

Nothing was formerly more striking in surgical experience than the difference in the behaviour of injuries according to whether the skin was implicated or not. Thus, if the bones of the leg were broken and the skin remained intact, the surgeon applied the necessary apparatus without any other anxiety than that of maintaining a good position of the fragments, although the internal injury to bones and soft parts might be very severe. If, on the other hand, a wound of the skin was present communicating with the broken bones, although the damage might be in other respects comparatively slight, the compound fracture, as it was termed, was one of the most dangerous accidents that could happen. Mr. Syme, who was, I believe, the safest surgeon of his time, once told me that he was inclined to think that it would be, on the whole, better if all compound fractures of the leg were subjected to amputation, without any attempt to save the limb. What was the cause of this astonishing difference ? It was clearly in some way due to the exposure of the injured parts to the external world. One obvious effect of such exposure was indicated by the odour of the discharge, which showed that the blood in the wound had undergone putrefactive change by which the bland nutrient liquid had been converted into highly irritating and poisonous substances. I have seen a man with compound fracture of the leg die within two days of the accident, as plainly poisoned by the products of putrefaction as if he had taken a fatal dose of some potent toxic drug.

An external wound of the soft parts might be healed in one of two ways. If its surfaces were clean cut and could be brought into accurate apposition, it might unite rapidly and *primarily*, 'by the first intention.' This, however, was exceptional. Too often the surgeon's efforts to obtain primary union were frustrated : the wound inflamed and the retentive stitches had to be removed, allowing it to gape ; and then, as if it had been left open from the first, healing had to be effected in the other way

which it is necessary for me briefly to describe. An exposed raw surface became covered in the first instance with a layer of clotted blood or certain of its constituents, which invariably putrefied ; and the irritation of the sensitive tissues by the putrid products appeared to me to account sufficiently for the inflammation which always occurred in and around an open wound during the three or four days which elapsed before what were termed 'granulations' had been produced. These constituted a coarsely granular coating of very imperfect or embryonic structure, destitute of sensory nerves and prone to throw off matter or pus, rather than absorb, as freshly divided tissues do, the products of putrefaction. The granulations thus formed a beautiful living plaster, which protected the sensitive parts beneath from irritation, and the system generally from poisoning and consequent febrile disturbance. The granulations had other useful properties of which I may mention their tendency to shrink as they grew, thus gradually reducing the dimensions of the sore. Meanwhile, another cause of its diminution was in operation. The cells of the epidermis or scarf-skin of the cutaneous margins were perpetually producing a crop of young cells of similar nature, which gradually spread over the granulations till they covered them entirely, and a complete cicatrix or scar was the result. Such was the other mode of healing, that by granulation and cicatrization ; a process which, when it proceeded unchecked to its completion, commanded our profound admiration. It was, however, essentially tedious compared with primary union, while, as we have seen, it was always preceded by more or less inflammation and fever, sometimes very serious in their effects. It was also liable to unforeseen interruptions. The sore might become larger instead of smaller, cicatrization giving place to ulceration in one of its various forms, or even to the frightful destruction of tissue which, from the circumstance that it was most frequently met with in hospitals, was termed hospital gangrene. Other serious and often fatal complications might arise, which the surgeon could only regard as untoward accidents and over which he had no efficient control.

It will be readily understood from the above description that the inflammation which so often frustrated the surgeon's endeavours after primary union was in my opinion essentially due to decomposition of blood within the wound.

These and many other considerations had long impressed me with the greatness of the evil of putrefaction in surgery. I had done my best to mitigate it by scrupulous ordinary cleanliness and the use of various deodorant lotions. But to prevent it altogether appeared hopeless while we believed with Liebig that its primary cause was the atmospheric oxygen which, in accordance with the researches of Graham, could not fail to be perpetually diffused through the porous dressings which were used to absorb the blood discharged from the wound. But when Pasteur had shown that putrefaction was a fermentation caused by the growth of microbes, and that these could not arise *de novo* in the

decomposable substance, the problem assumed a more hopeful aspect. If the wound could be treated with some substance which, without doing too serious mischief to the human tissues, would kill the microbes already contained in it and prevent the future access of others in the living state, putrefaction might be prevented, however freely the air with its oxygen might enter. I had heard of carbolic acid as having a remarkable deodorising effect upon sewage, and having obtained from my colleague Dr. Anderson, Professor of Chemistry in the University of Glasgow, a sample which he had of this product, then little more than a chemical curiosity in Scotland, I determined to try it in compound fractures. Applying it undiluted to the wound, with an arrangement for its occasional renewal, I had the joy of seeing these formidable injuries follow the same safe and tranquil course as simple fractures, in which the skin remains unbroken.

At the same time we had the intense interest of observing in open wounds what had previously been hidden from human view, the manner in which subcutaneous injuries are repaired. Of special interest was the process by which portions of tissue killed by the violence of the accident were disposed of, as contrasted with what had till then been invariably witnessed. Dead parts had been always seen to be gradually separated from the living by an inflammatory process and thrown off as sloughs. But when protected by the antiseptic dressing from becoming putrid and therefore irritating, a structure deprived of its life caused no disturbance in its vicinity ; and, on the contrary, being of a nutritious nature, it served as pabulum for the growing elements of the neighbouring living structures, and these became in due time entirely substituted for it. Even dead bone was seen to be thus replaced by living osseous tissue.

This suggested the idea of using threads of dead animal tissue for tying blood-vessels ; and this was realised by means of catgut, which is made from the intestine of the sheep. If deprived of living microbes, and otherwise properly prepared, catgut answers its purpose completely ; the knot holding securely, while the ligature around the vessel becomes gradually absorbed and replaced by a ring of living tissue. The threads, instead of being left long as before, could now be cut short, and the tedious process of separation of the ligature, with its attendant serious danger of bleeding, was avoided.

Undiluted carbolic acid is a powerful caustic ; and although it might be employed in compound fracture, where some loss of tissue was of little moment in comparison with the tremendous danger to be averted, it was altogether unsuitable for wounds made by the surgeon. It soon appeared, however, that the acid would answer the purpose aimed at, though used in diluted forms devoid of caustic action, and therefore applicable to operative surgery. According to our then existing knowledge, two essential points had to be aimed at : to conduct the operation so that on its completion the wound should contain no living microbes, and to apply a

dressing capable of preventing the access of other living organisms till the time should have arrived for changing it.

Carbolic acid lent itself well to both these objects. Our experience with this agent brought out what was, I believe, a new principle in pharmacology—namely, that the energy of action of any substance upon the human tissues depends not only upon the proportion in which it is contained in the material used as a vehicle for its administration, but also upon the degree of tenacity with which it is held by its solvent. Water dissolves carbolic acid sparingly and holds it extremely lightly, leaving it free to act energetically on other things for which it has greater affinity, while various organic substances absorb it greedily and hold it tenaciously. Hence its watery solution seemed admirably suited for a detergent lotion to be used during the operation for destroying any microbes that might fall upon the wound, and for purifying the surrounding skin and also the surgeon's hands and instruments. For the last-named purpose it had the further advantage that it did not act on steel.

For an external dressing the watery solution was not adapted, as it soon lost the acid it contained, and was irritating while it lasted. For this purpose some organic substances were found to answer well. Large proportions of the acid could be blended with them in so bland a form as to be unirritating; and such mixtures, while perpetually giving off enough of the volatile salt to prevent organic development in the discharges that flowed past them, served as a reliable store of the antiseptic for days together.

The appliances which I first used for carrying out the antiseptic principle were both rude and needlessly complicated. The years that have since passed have witnessed great improvements in both respects. Of the various materials which have been employed by myself and others, and their modes of application, I need say nothing except to express my belief, as a matter of long experience, that carbolic acid, by virtue of its powerful affinity for the epidermis and oily matters associated with it, and also its great penetrating power, is still the best agent at our disposal for purifying the skin around the wound. But I must say a few words regarding a most important simplification of our procedure. Pasteur, as we have seen, had shown that the air of every inhabited room teems with microbes; and for a long time I employed various more or less elaborate precautions against the living atmospheric dust, not doubting that, as all wounds except the few which healed completely by the first intention underwent putrefactive fermentation, the blood must be a peculiarly favourable soil for the growth of putrefactive microbes. But I afterwards learnt that such was by no means the case. I had performed many experiments in confirmation of Pasteur's germ theory, not indeed in order to satisfy myself of its truth, but in the hope of convincing others. I had observed that uncontaminated milk, which would remain unaltered for an indefinite time if protected from dust,

was made to teem with microbes of different kinds by a very brief exposure to the atmosphere, and that the same effect was produced by the addition of a drop of ordinary water. But when I came to experiment with blood drawn with antiseptic precautions into sterilised vessels, I saw to my surprise that it might remain free from microbes in spite of similar access of air or treatment with water. I even found that if very putrid blood was largely diluted with sterilised water, so as to diffuse its microbes widely and wash them of their acrid products, a drop of such dilution added to pure blood might leave it unchanged for days at the temperature of the body, although a trace of the septic liquid undiluted caused intense putrefaction within twenty-four hours. Hence I was led to conclude that it was the grosser forms of septic mischief, rather than microbes in the attenuated condition in which they existed in the atmosphere, that we had to dread in surgical practice. And at the London Medical Congress in 1881, I hinted, when describing the experiments I have alluded to, that it might turn out possible to disregard altogether the atmospheric dust. But greatly as I should have rejoiced at such a simplification of our procedure, if justifiable, I did not then venture to test it in practice. I knew that with the safeguards which we then employed I could ensure the safety of my patients, and I did not dare to imperil it by relaxing them. There is one golden rule for all experiments upon our fellow-men. Let the thing tried be that which, according to our best judgment, is the most likely to promote the welfare of the patient. In other words, Do as you would be done by.

Nine years later, however, at the Berlin Congress in 1890, I was able to bring forward what was, I believe, absolute demonstration of the harmlessness of the atmospheric dust in surgical operations. This conclusion has been justified by subsequent experience : the irritation of the wound by antiseptic irrigation and washing may therefore now be avoided, and nature left quite undisturbed to carry out her best methods of repair, while the surgeon may conduct his operations as simply as in former days, provided always that, deeply impressed with the tremendous importance of his object, and inspiring the same conviction in all his assistants, he vigilantly maintains from first to last, with a care that, once learnt, becomes instinctive, but for the want of which nothing else can compensate, the use of the simple means which will suffice to exclude from the wound the coarser forms of septic impurity.

Even our earlier and ruder methods of carrying out the antiseptic principle soon produced a wonderful change in my surgical wards in the Glasgow Royal Infirmary, which, from being some of the most unhealthy in the kingdom, became, as I believe I may say without exaggeration, the healthiest in the world ; while other wards, separated from mine only by a passage a few feet broad, where former modes of treatment were for a while continued, retained their former insalubrity. This result, I need hardly remark, was not in any degree due to special skill on my part, but simply

to the strenuous endeavour to carry out strictly what seemed to me a principle of supreme importance.

Equally striking changes were afterwards witnessed in other institutions. Of these I may give one example. In the great Allgemeines Krankenhaus of Munich, hospital gangrene had become more and more rife from year to year, till at length the frightful condition was reached that 80 per cent. of all wounds became affected by it. It is only just to the memory of Professor von Nussbaum, then the head of that establishment, to say that he had done his utmost to check this frightful scourge ; and that the evil was not caused by anything peculiar in his management was shown by the fact that in a private hospital under his care there was no unusual unhealthiness. The larger institution seemed to have become hopelessly infected, and the city authorities were contemplating its demolition and reconstruction. Under these circumstances, Professor von Nussbaum despatched his chief assistant, Dr. Lindpaintner, to Edinburgh, where I at that time occupied the chair of clinical surgery, to learn the details of the antiseptic system as we then practised it. He remained until he had entirely mastered them, and after his return all the cases were on a certain day dressed on our plan. From that day forward not a single case of hospital gangrene occurred in the Krankenhaus. The fearful disease pyæmia likewise disappeared, and erysipelas soon followed its example.

But it was by no means only in removing the unhealthiness of hospitals that the antiseptic system showed its benefits. Inflammation being suppressed, with attendant pain, fever, and wasting discharge, the sufferings of the patient were, of course, immensely lessened ; rapid primary union being now the rule, convalescence was correspondingly curtailed ; while as regards safety and the essential nature of the mode of repair, it became a matter of indifference whether the wound had clean-cut surfaces which could be closely approximated, or whether the injury inflicted had been such as to cause destruction of tissue. And operations which had been regarded from time immemorial as unjustifiable were adopted with complete safety.

It pleases me to think that there is an ever-increasing number of practitioners throughout the world to whom this will not appear the language of exaggeration. There are cases in which, from the situation of the part concerned or other unusual circumstances, it is impossible to carry out the antiseptic system completely. These, however, are quite exceptional ; and even in them much has been done to mitigate the evil which cannot be altogether avoided.

I ask your indulgence if I have seemed to dwell too long upon matters in which I have been personally concerned. I now gladly return to the labours of others.

The striking results of the application of the germ theory to Surgery acted as a powerful stimulus to the investigation of the nature of the

micro-organisms concerned ; and it soon appeared that putrefaction was by no means the only evil of microbic origin to which wounds were liable. I had myself very early noticed that hospital gangrene was not necessarily attended by any unpleasant odour ; and I afterwards made a similar observation regarding the matter formed in a remarkable epidemic of erysipelas in Edinburgh obviously of infective character. I had also seen a careless dressing followed by the occurrence of suppuration without putrefaction. And as these non-putrefactive disorders had the same self-propagating property as ferments, and were suppressed by the same anti-septic agencies which were used for combating the putrefactive microbes, I did not doubt that they were of an analogous origin ; and I ventured to express the view that, just as the various fermentations had each its special microbe, so it might be with the various complications of wounds. This surmise was afterwards amply verified. Professor Ogston, of Aberdeen, was an early worker in this field, and showed that in acute abscesses, that is to say those which run a rapid course, the matter, although often quite free from unpleasant odour, invariably contains micro-organisms belonging to the group which, from the spherical form of their elements, are termed micrococci ; and these he classed as streptococci or staphylococci, according as they were arranged in chains or disposed in irregular clusters like bunches of grapes. The German pathologist, Fehleisen, followed with a beautiful research, by which he clearly proved that erysipelas is caused by a streptococcus. A host of earnest workers in different countries have cultivated the new science of Bacteriology, and, while opening up a wide fresh domain of Biology, have demonstrated in so many cases the causal relation between special micro-organisms and special diseases, not only in wounds but in the system generally, as to afford ample confirmation of the induction which had been made by Pasteur that all infective disorders are of microbic origin.

Not that we can look forward with anything like confidence to being able ever to see the *materies morbi* of every disease of this nature. One of the latest of such discoveries has been that by Pfeiffer of Berlin of the bacillus of influenza, perhaps the most minute of all micro-organisms ever yet detected. The bacillus of anthrax, the cause of a plague common among cattle in some parts of Europe, and often communicated to sorters of foreign wool in this country, is a giant as compared with this tiny being ; and supposing the microbe of any infectious fever to be as much smaller than the influenza bacillus as this is less than that of anthrax, a by no means unlikely hypothesis, it is probable that it would never be visible to man. The improvements of the microscope, based on the principle established by my father in the earlier part of the century, have apparently nearly reached the limits of what is possible. But that such parasites are really the causes of all this great class of diseases can no longer be doubted.

The first rational step towards the prevention or cure of disease is to

know its cause ; and it is impossible to over-estimate the practical value of researches such as those to which I am now referring. Among their many achievements is what may be fairly regarded as the most important discovery ever made in pathology, because it revealed the true nature of the disease which causes more sickness and death in the human race than any other. It was made by Robert Koch, who greatly distinguished himself, when a practitioner in an obscure town in Germany, by the remarkable combination of experimental acuteness and skill, chemical and optical knowledge and successful micro-photography which he brought to bear upon the illustration of infective diseases of wounds in the lower animals ; in recognition of which service the enlightened Prussian Government at once appointed him to an official position of great importance in Berlin. There he conducted various important researches ; and at the London Congress in 1881 he showed to us for the first time the bacillus of tubercle. Wonderful light was thrown by this discovery upon a great group of diseases which had before been rather guessed than known to be of an allied nature ; a precision and efficacy never before possible was introduced into their surgical treatment, while the physician became guided by new and sure light as regards their diagnosis and prevention.

At that same London Congress Koch demonstrated to us his 'plate culture' of bacteria, which was so important that I must devote a few words to its description. With a view to the successful study of the habits and effects of any particular microbe outside the living body, it is essential that it should be present unmixed in the medium in which it is cultivated. It can be readily understood how difficult it must have been to isolate any particular micro-organism when it existed mixed, as was often the case, with a multitude of other forms. In fact, the various ingenious attempts made to effect this object had often proved entire failures. Koch, however, by an ingenious procedure converted what had been before impossible into a matter of the utmost facility. In the broth or other nutrient liquid which was to serve as food for the growing microbe he dissolved, by aid of heat, just enough gelatine to ensure that, while it should become a solid mass when cold, it should remain fluid though reduced in temperature so much as to be incapable of killing living germs. To the medium thus partially cooled was added some liquid containing, among others, the microbe to be investigated ; and the mixture was thoroughly shaken so as to diffuse the bacteria and separate them from each other. Some of the liquid was then poured out in a thin layer upon a glass plate and allowed to cool so as to assume the solid form. The various microbes, fixed in the gelatine and so prevented from intermingling, proceeded to develop each its special progeny, which in course of time showed itself as an opaque speck in the transparent film. Any one of such specks could now be removed and transferred to another vessel in which the microbe composing it grew in perfect isolation.

Pasteur was present at this demonstration, and expressed his sense of

the great progress effected by the new method. It was soon introduced into his own institute and other laboratories throughout the world ; and it has immensely facilitated bacteriological study.

One fruit of it in Koch's own hands was the discovery of the microbe of cholera in India, whither he went to study the disease. This organism was termed by Koch from its curved form the 'comma bacillus,' and by the French the cholera vibrio. Great doubts were for a long time felt regarding this discovery. Several other kinds of bacteria were found of the same shape, some of them producing very similar appearances in culture media. But bacteriologists are now universally agreed that, although various other conditions are necessary to the production of an attack of cholera besides the mere presence of the vibrio, yet it is the essential *materies morbi* ; and it is by the aid of the diagnosis which its presence in any case of true cholera enables the bacteriologist to make, that threatened invasions of this awful disease have of late years been so successfully repelled from our shores. If bacteriology had done nothing more for us than this, it might well have earned our gratitude.

I have next to invite your attention to some earlier work of Pasteur. There is a disease known in France under the name of *choléra des poules*, which often produced great havoc among the poultry yards of Paris. It had been observed that the blood of birds that had died of this disease was peopled by a multitude of minute bacteria, not very dissimilar in form and size to the microbe of the lactic ferment to which I have before referred. And Pasteur found that, if this bacterium was cultivated outside the body for a protracted period under certain conditions, it underwent a remarkable diminution of its virulence ; so that, if inoculated into a healthy fowl, it no longer caused the death of the bird, as it would have done in its original condition, but produced a milder form of the disease which was not fatal. And this altered character of the microbe, caused by certain conditions, was found to persist in successive generations cultivated in the ordinary way. Thus was discovered the great fact of what Pasteur termed the *atténuation des virus*, which at once gave the clue to understanding what had before been quite mysterious, the difference in virulence of the same disease in different epidemics.

But he made the further very important observation that a bird which had gone through the mild form of the complaint had acquired immunity against it in its most virulent condition. Pasteur afterwards succeeded in obtaining mitigated varieties of microbes for some other diseases ; and he applied with great success the principle which he had discovered in fowl-cholera for protecting the larger domestic animals against the plague of anthrax. The preparations used for such preventive inoculations he termed 'vaccins' in honour of our great countryman, Edward Jenner. For Pasteur at once saw the analogy between the immunity to fowl-cholera produced by its attenuated virus and the protection afforded against small-pox by vaccination. And while pathologists still hesitated,

he had no doubt of the correctness of Jenner's expression *variola vaccinae*, or small-pox in the cow.

It is just a hundred years since Jenner made the crucial experiment of inoculating with small-pox a boy whom he had previously vaccinated, the result being, as he anticipated, that the boy was quite unaffected. It may be remarked that this was a perfectly legitimate experiment, involving no danger to the subject of it. Inoculation was at that time the established practice ; and if vaccination should prove nugatory, the inoculation would be only what would have been otherwise called for ; while it would be perfectly harmless if the hoped-for effect of vaccination had been produced.

We are a practical people, not much addicted to personal commemorations : although our nation did indeed celebrate with fitting splendour the jubilee of the reign of our beloved Queen ; and at the invitation of Glasgow the scientific world has lately marked in a manner, though different, as imposing, the jubilee of the life-work of a sovereign in science (Lord Kelvin). But while we cannot be astonished that the centenary of Jenner's immortal discovery should have failed to receive general recognition in this country, it is melancholy to think that this year should, in his native county, have been distinguished by a terrible illustration of the results which would sooner or later inevitably follow the general neglect of his prescriptions.

I have no desire to speak severely of the Gloucester Guardians. They are not sanitary authorities, and had not the technical knowledge necessary to enable them to judge between the teachings of true science and the declamations of misguided, though well-meaning, enthusiasts. They did what they believed to be right ; and when roused to a sense of the greatness of their mistake, they did their very best to repair it, so that their city is said to be now the best vaccinated in Her Majesty's dominions. But though by their praiseworthy exertions they succeeded in promptly checking the raging epidemic, they cannot recall the dead to life, or restore beauty to marred features, or sight to blinded eyes. Would that the entire country and our Legislature might take duly to heart this object-lesson !

How completely the medical profession were convinced of the efficacy of vaccination in the early part of this century was strikingly illustrated by an account given by Professor Crookshank, in his interesting history of this subject, of several eminent medical men in Edinburgh meeting to see the to them unprecedented fact of a vaccinated person having taken small-pox. It has, of course, since become well known that the milder form of the disease, as modified by passing through the cow, confers a less permanent protection than the original human disorder. This it was, of course, impossible for Jenner to foresee. It is, indeed, a question of degree, since a second attack of ordinary small-pox is occasionally known to occur, and vaccination, long after it has ceased to give perfect immunity, greatly modifies the character of the disorder and diminishes its

danger. And, happily, in re-vaccination after a certain number of years we have the means of making Jenner's work complete. I understand that the majority of the Commissioners, who have recently issued their report upon this subject, while recognising the value and importance of re-vaccination, are so impressed with the difficulties that would attend making it compulsory by legislation that they do not recommend that course; although it is advocated by two of their number who are of peculiarly high authority on such a question. I was lately told by a Berlin professor that no serious difficulty is experienced in carrying out the compulsory law that prevails in Germany. The masters of the schools are directed to ascertain in the case of every child attaining the age of twelve whether re-vaccination has been practised. If not, and the parents refuse to have it done, they are fined one mark. If this does not prove effectual, the fine is doubled: and if even the double penalty should not prove efficacious, a second doubling of it would follow, but, as my informant remarked, it is very seldom that it is called for. The result is that small-pox is a matter of extreme rarity in that country; while it is absolutely unknown in the huge German army, in consequence of the rule that every soldier is re-vaccinated on entering the service. Whatever view our Legislature may take on this question, one thing seems to me clear: that it will be the duty of Government to encourage by every available means the use of calf lymph, so as to exclude the possibility of the communication of any human disease to the child, and to institute such efficient inspection of vaccination institutes as shall ensure careful antiseptic arrangements, and so prevent contamination by extraneous microbes. If this were done, 'conscientious objections' would cease to have any rational basis. At the same time, the administration of the regulations on vaccination should be transferred (as advised by the Commissioners) to competent sanitary authorities.

But to return to Pasteur. In 1880 he entered upon the study of that terrible but then most obscure disease, Hydrophobia or Rabies, which from its infective character he was sure must be of microbic origin, although no micro-organism could be detected in it. He early demonstrated the new pathological fact that the virus had its essential seat in the nervous system. This proved the key to his success in this subject. One result that flowed from it has been the cause of unspeakable consolation to many. The foolish practice is still too prevalent of killing the dog that has bitten any one, on the absurd notion that, if it were mad, its destruction would prevent the occurrence of Hydrophobia in the person bitten. The idea of the bare possibility of the animal having been so affected causes an agony of suspense during the long weeks or months of possible incubation of the disease. Very serious nervous symptoms aping true Hydrophobia have been known to result from the terror thus inspired. Pasteur showed that if a little of the brain or spinal cord of a dog that had been really mad was inoculated in an appropriate manner into a rabbit, it

infallibly caused rabies in that animal in a few days. If therefore such an experiment was made with a negative result, the conclusion might be drawn with certainty that the dog had been healthy. It is perhaps right that I should say that the inoculation is painlessly done under an anæsthetic, and that in the rabbit rabies does not assume the violent form that it does in the dog, but produces gradual loss of power with little if any suffering.

This is the more satisfactory because rabbits in which the disease has been thus artificially induced are employed in carrying out what was Pasteur's greatest triumph, the preventive treatment of Hydrophobia in the human subject. We have seen that Pasteur discovered that microbes might under some circumstances undergo mitigation of their virulence. He afterwards found that under different conditions they might have it exalted, or, as he expressed it, there might be a *renforcement du virus*. Such proved to be the case with rabies in the rabbit; so that the spinal cords of animals which had died of it contained the poison in a highly intensified condition. But he also found that if such a highly virulent cord was suspended under strict antiseptic precautions in a dry atmosphere at a certain temperature, it gradually from day to day lost in potency, till in course of time it became absolutely inert. If now an emulsion of such a harmless cord was introduced under the skin of an animal, as in the subcutaneous administration of morphia, it might be followed without harm another day by a similar dose of a cord still rather poisonous; and so from day to day stronger and stronger injections might be used, the system becoming gradually accustomed to the poison, till a degree of virulence had been reached far exceeding that of the bite of a mad dog. When this had been attained, the animal proved incapable of taking the disease in the ordinary way; and more than that, if such treatment was adopted after an animal had already received the poison, provided that too long a time had not elapsed, the outbreak of the disease was prevented. It was only after great searching of heart that Pasteur, after consultation with some trusted medical friends, ventured upon trying this practice upon man. It has since been extensively adopted in various parts of the world with increasing success as the details of the method were improved. It is not of course the case that every one bitten by a really rabid animal takes the disease; but the percentage of those who do so, which was formerly large, has been reduced almost to zero by this treatment, if not too long delayed.

While the intensity of rabies in the rabbit is undoubtedly due to a peculiarly virulent form of the microbe concerned, we cannot suppose that the daily diminishing potency of the cord suspended in dry warm air is an instance of attenuation of virus, using the term 'virus' as synonymous with the microbe concerned. In other words, we have no reason to believe that the special micro-organism of hydrophobia continues to develop in the dead cord and produce successively a milder and milder

progeny, since rabies cannot be cultivated in the nervous system of a dead animal. We must rather conclude that there must be some chemical poison present which gradually loses its potency as time passes. And this leads me to refer to another most important branch of this large subject of bacteriology, that of the poisonous products of microbes.

It was shown several years ago by Roux and Yersin, working in the *Institut Pasteur*, that the crust or false membrane which forms upon the throats of patients affected with diphtheria contains bacteria which can be cultivated outside the body in a nutrient liquid, with the result that it acquires poisonous qualities of astonishing intensity, comparable to that of the secretion of the poison-glands of the most venomous serpents. And they also ascertained that the liquid retained this property after the microbes had been removed from it by filtration, which proved that the poison must be a chemical substance in solution, as distinguished from the living element which had produced it. These poisonous products of bacteria, or toxins as they have been termed, explain the deadly effects of some microbes, which it would otherwise be impossible to understand. Thus, in diphtheria itself the special bacillus which was shown by Löffler to be its cause, does not become propagated in the blood, like the microbe of chicken cholera, but remains confined to the surface on which it first appeared : but the toxin which it secretes is absorbed from that surface into the blood, and so poisons the system. Similar observations have been made with regard to the microbes of some other diseases, as, for example, the bacillus of tetanus or lockjaw. This remains localised in the wound, but forms a special toxin of extreme potency, which becomes absorbed and diffused through the body.

Wonderful as it seems, each poisonous microbe appears to form its own peculiar toxin. Koch's tuberculin was of this nature, a product of the growth of the tubercle bacillus in culture media. Here, again, great effects were produced by extremely minute quantities of the substance, but here a new peculiarity showed itself, viz. that patients affected with tubercular disease, in any of its varied forms, exhibited inflammation in the affected part and general fever after receiving under the skin an amount of the material which had no effect whatever upon healthy persons. I witnessed in Berlin some instances of these effects, which were simply astounding. Patients affected with a peculiar form of obstinate ulcer of the face showed, after a single injection of the tuberculin, violent inflammatory redness and swelling of the sore and surrounding skin ; and, what was equally surprising, when this disturbance subsided the disease was found to have undergone great improvement. By repetitions of such procedures, ulcers which had previously been steadily advancing, in spite of ordinary treatment, became greatly reduced in size, and in some instances apparently cured. Such results led Koch to believe that he had obtained an effectual means of dealing with tubercular disease in all its forms. Unhappily, the apparent cure proved to be only of

transient duration, and the high hopes which had been inspired by Koch's great reputation were dashed. It is but fair to say that he was strongly urged to publish before he was himself disposed to do so, and we cannot but regret that he yielded to the pressure put upon him.

But though Koch's sanguine anticipations were not realised, it would be a great mistake to suppose that his labours with tuberculin have been fruitless. Cattle are liable to tubercle, and, when affected with it, may become a very serious source of infection for human beings, more especially when the disease affects the udders of cows, and so contaminates the milk. By virtue of the close affinity that prevails between the lower animals and ourselves, in disease as well as in health, tuberculin produces fever in tubercular cows in doses which do not affect healthy beasts. Thus, by the subcutaneous use of a little of the fluid, tubercle latent in internal organs of an apparently healthy cow can be with certainty revealed, and the slaughter of the animal after this discovery protects man from infection.

It has been ascertained that glanders presents a precise analogy with tubercle as regards the effects of its toxic products. If the microbe which has been found to be the cause of this disease is cultivated in appropriate media, it produces a poison which has received the name of mallein, and the subcutaneous injection of a suitable dose of this fluid into a glandered horse causes striking febrile symptoms which do not occur in a healthy animal. Glanders, like tubercle, may exist in insidious latent forms which there was formerly no means of detecting, but which are at once disclosed by this means. If a glandered horse has been accidentally introduced into a large stable, this method of diagnosis surely tells if it has infected others. All receive a little mallein. Those which become affected with fever are slaughtered, and thus not only is the disease prevented from spreading to other horses, but the grooms are protected from a mortal disorder.

This valuable resource sprang from Koch's work on tuberculin, which has also indirectly done good in other ways. His distinguished pupil, Behring, has expressly attributed to those researches the inspiration of the work which led him and his since famous collaborateur, the Japanese Kitasato, to their surprising discovery of anti-toxic serum. They found that if an animal of a species liable to diphtheria or tetanus received a quantity of the respective toxin, so small as to be harmless, and afterwards, at suitable intervals, successively stronger and stronger doses, the creature, in course of time, acquired such a tolerance for the poison as to be able to receive with impunity a quantity very much greater than would at the outset have proved fatal. So far, we have nothing more than seems to correspond with the effects of the increasingly potent cords in Pasteur's treatment of rabies. But what was entirely new in their results was that, if blood was drawn from an animal which had acquired this high degree of artificial immunity, and some of the clear fluid or serum which exuded from it after it had clotted was introduced under the

skin of another animal, this second animal acquired a strong, though more transient, immunity against the particular toxin concerned. The serum in some way counteracted the toxin or was antitoxic. But, more than that, if some of the antitoxic serum was applied to an animal after it had already received a poisonous dose of the toxin, it preserved the life of the creature, provided that too long a time had not elapsed after the poison was introduced. In other words, the antitoxin proved to be not only preventive but curative.

Similar results were afterwards obtained by Ehrlich, of Berlin, with some poisons not of bacterial origin, but derived from the vegetable kingdom; and quite recently the independent labours of Calmette of Lille and Fraser of Edinburgh have shown that antidotes of wonderful efficacy against the venom of serpents may be procured on the same principle. Calmette has obtained antitoxin so powerful that a quantity of it only a 200,000th part of the weight of an animal will protect it perfectly against a dose of the secretion of the poison-glands of the most venomous serpents known to exist, which without such protection would have proved fatal in four hours. For curative purposes larger quantities of the remedy are required, but cases have been already published by Calmette in which death appears to have been averted in the human subject by this treatment.

Behring's darling object was to discover means of curing tetanus and diphtheria in man. In tetanus the conditions are not favourable; because the specific bacilli lurk in the depths of the wound, and only declare their presence by symptoms caused by their toxin having been already in a greater or less amount diffused through the system; and in every case of this disease there must be a fear that the antidote may be applied too late to be useful. But in diphtheria the bacilli very early manifest their presence by the false membrane which they cause upon the throat, so that the antitoxin has a fair chance; and here we are justified in saying that Behring's object has been attained.

The problem, however, was by no means so simple as in the case of some mere chemical poison. However effectual the antitoxin might be against the toxin, if it left the bacilli intact, not only would repeated injections be required to maintain the transient immunity to the poison perpetually secreted by the microbes, but the bacilli might by their growth and extension cause obstruction of the respiratory passages.

Roux, however, whose name must always be mentioned with honour in relation to this subject, effectually disposed of this difficulty. He showed by experiments on animals that a diphtheritic false membrane, rapidly extending and accompanied by surrounding inflammation, was brought to a stand by the use of the antitoxin, and soon dropped off, leaving a healthy surface. Whatever be the explanation, the fact was thus established that the antitoxic serum, while it renders the toxin harmless, causes the microbe to languish and disappear.

No theoretical objection could now be urged against the treatment;

and it has during the last two years been extensively tested in practice in various parts of the world, and it has gradually made its way more and more into the confidence of the profession. One important piece of evidence in its favour in this country is derived from the report of the six large hospitals under the management of the London Asylums Board. The medical officers of these hospitals at first naturally regarded the practice with scepticism : but as it appeared to be at least harmless, they gave it a trial ; and during the year 1895 it was very generally employed upon the 2,182 cases admitted ; and they have all become convinced of its great value. In the nature of things, if the theory of the treatment is correct, the best results must be obtained when the patients are admitted at an early stage of the attack, before there has been time for much poisoning of the system : and accordingly we learn from the report that, comparing 1895 with 1894, during which latter year the ordinary treatment had been used, the percentage of mortality, in all the six hospitals combined, among the patients admitted on the first day of the disease, which in 1894 was 22·5, was only 4·6 in 1895 ; while for those admitted on the second day the numbers are 27 for 1894 and 14·8 for 1895. Thus for cases admitted on the first day the mortality was only one-fifth of what it was in the previous year, and for those entering on the second it was halved. Unfortunately in the low parts of London which furnish most of these patients the parents too often delay sending in the children till much later : so that on the average no less than 67·5 per cent. were admitted on the fourth day of the disease or later. Hence the aggregate statistics of all cases are not nearly so striking. Nevertheless, taking it altogether, the mortality in 1895 was less than had ever before been experienced in those hospitals. I should add that there was no reason to think that the disease was of a milder type than usual in 1895 ; and no change whatever was made in the treatment except as regards the antitoxic injections.

There is one piece of evidence recorded in the report which, though it is not concerned with high numbers, is well worthy of notice. It relates to a special institution to which convalescents from scarlet fever are sent from all the six hospitals. Such patients occasionally contract diphtheria, and when they do so the added disease has generally proved extremely fatal. In the five years preceding the introduction of the treatment with antitoxin the mortality from this cause had never been less than 50 per cent, and averaged on the whole 61·9 per cent. During 1895, under antitoxin, the deaths among the 119 patients of this class were only 7·5 per cent., or one-eighth of what had been previously experienced. This very striking result seems to be naturally explained by the fact that these patients being already in hospital when the diphtheria appeared, an unusually early opportunity was afforded for dealing with it.

There are certain cases of so malignant a character from the first that no treatment will probably ever be able to cope with them. But taking

all cases together it seems probable that Behring's hope that the mortality may be reduced to 5 per cent. will be fully realised when the public become alive to the paramount importance of having the treatment commenced at the outset of the disease.

There are many able workers in the field of Bacteriology whose names time does not permit me to mention, and to whose important labours I cannot refer; and even those researches of which I have spoken have been, of course, most inadequately dealt with. I feel this especially with regard to Pasteur, whose work shines out more brightly the more his writings are perused.

I have lastly to bring before you a subject which, though not bacteriological, has intimate relations with bacteria. If a drop of blood is drawn from the finger by a prick with a needle and examined microscopically between two plates of glass, there are seen in it minute solid elements of two kinds, the one pale orange bi-concave discs, which, seen in mass, give the red colour to the vital fluid, the other more or less granular spherical masses of the soft material called protoplasm, destitute of colour, and therefore called the colourless or white corpuscles. It has been long known that if the microscope was placed at such a distance from a fire as to have the temperature of the human body, the white corpuscles might be seen to put out and retract little processes or pseudopodia, and by their means crawl over the surface of the glass, just like the extremely low forms of animal life termed, from this faculty of changing their form, *amoebæ*. It was a somewhat weird spectacle, that of seeing what had just before been constituents of our own blood moving about like independent creatures. Yet there was nothing in this inconsistent with what we knew of the fixed components of the animal frame. For example, the surface of a frog's tongue is covered with a layer of cells, each of which is provided with two or more lashing filaments or cilia, and those of all the cells acting in concert cause a constant flow of fluid in a definite direction over the organ. If we gently scrape the surface of the animal's tongue, we can detach some of these ciliated cells; and on examining them with the microscope in a drop of water, we find that they will continue for an indefinite time their lashing movements, which are just as much living or vital in their character as the writhings of a worm. And, as I observed many years ago, these detached cells behave under the influence of a stimulus just like parts connected with the body, the movements of the cilia being excited to greater activity by gentle stimulation, and thrown into a state of temporary inactivity when the irritation was more severe. Thus each constituent element of our bodies may be regarded as in one sense an independent living being, though all work together in marvellous harmony for the good of the body politic. The independent movements of the white corpuscles outside the body were therefore not astonishing: but they long remained matters of mere curiosity. Much interest was called to them by the observation of the German pathologist Cohnheim that in some

inflammatory conditions they passed through the pores in the walls of the finest blood-vessels, and thus escaped into the interstices of the surrounding tissues. Cohnheim attributed their transit to the pressure of the blood. But why it was that, though larger than the red corpuscles, and containing a nucleus which the red ones have not, they alone passed through the pores of the vessels, or why it was that this emigration of the white corpuscles occurred abundantly in some inflammations and was absent in others, was quite unexplained.

These white corpuscles, however, have been invested with extraordinary new interest by the researches of the Russian naturalist and pathologist, Metchnikoff. He observed that, after passing through the walls of the vessels, they not only crawl about like *amœbæ*, but, like them, receive nutritious materials into their soft bodies and digest them. It is thus that the effete materials of a tadpole's tail are got rid of ; so that they play a most important part in the function of absorption.

But still more interesting observations followed. He found that a microscopic crustacean, a kind of water-flea, was liable to be infested by a fungus which had exceedingly sharp-pointed spores. These were apt to penetrate the coats of the creature's intestine, and project into its body-cavity. No sooner did this occur with any spore than it became surrounded by a group of the cells which are contained in the cavity of the body and correspond to the white corpuscles of our blood. These proceeded to attempt to devour the spore ; and if they succeeded, in every such case the animal was saved from the invasion of the parasite. But if the spores were more than could be disposed of by the devouring cells (phagocytes, as Metchnikoff termed them), the water-flea succumbed.

Starting from this fundamental observation, he ascertained that the microbes of infective diseases are subject to this same process of devouring and digestion, carried on both by the white corpuscles and by cells that line the blood-vessels. And by a long series of most beautiful researches he has, as it appears to me, firmly established the great truth that phagocytosis is the main defensive means possessed by the living body against the invasions of its microscopic foes. The power of the system to produce antitoxic substances to counteract the poisons of microbes is undoubtedly in its own place of great importance. But in the large class of cases in which animals are naturally refractory to particular infective diseases the blood is not found to yield any antitoxic element by which the natural immunity can be accounted for. Here phagocytosis seems to be the sole defensive agency. And even in cases in which the serum does possess antitoxic, or, as it would seem in some cases, germicidal properties, the bodies of the dead microbes must at last be got rid of by phagocytosis, and some recent observations would seem to indicate that the useful elements of the serum may be, in part at least, derived from the digestive juices of the phagocytes. If ever there was a romantic chapter in pathology, it has surely been that of the story of phagocytosis.

I was myself peculiarly interested by these observations of Metchnikoff's, because they seemed to me to afford clear explanation of the healing of wounds by first intention under circumstances before incomprehensible. This primary union was sometimes seen to take place in wounds treated with water-dressing, that is to say, a piece of wet lint covered with a layer of oiled-silk to keep it moist. This, though cleanly when applied, was invariably putrid within twenty-four hours. The layer of blood between the cut surfaces was thus exposed at the outlet of the wound to a most potent septic focus. How was it prevented from putrefying, as it would have done under such influence if, instead of being between divided living tissues, it had been between plates of glass or other indifferent material? Pasteur's observations pushed the question a step further. It now was, How were the bacteria of putrefaction kept from propagating in the decomposable film? Metchnikoff's phagocytosis supplied the answer. The blood between the lips of the wound became rapidly peopled with phagocytes, which kept guard against the putrefactive microbes and seized them as they endeavoured to enter.

If phagocytosis was ever able to cope with septic microbes in so concentrated and intense a form, it could hardly fail to deal effectually with them in the very mitigated condition in which they are present in the air. We are thus strongly confirmed in our conclusion that the atmospheric dust may safely be disregarded in our operations: and Metchnikoff's researches, while they have illumined the whole pathology of infective diseases, have beautifully completed the theory of antiseptic treatment in surgery.

I might have taken equally striking illustration of my theme from other departments in which microbes play no part. In fact any attempt to speak of all that the art of healing has borrowed from science and contributed to it during the past half-century would involve a very extensive dissertation on pathology and therapeutics. I have culled specimens from a wide field; and I only hope that in bringing them before you I have not overstepped the bounds of what is fitting before a mixed company. For many of you my remarks can have had little if any novelty: for others they may perhaps possess some interest as showing that Medicine is no unworthy ally of the British Association—that, while her practice is ever more and more based on science, the ceaseless efforts of her votaries to improve what have been fittingly designated *Quæ prosumunt omnibus artes*, are ever adding largely to the sum of abstract knowledge.

British Association for the Advancement of Science.

TORONTO, 1897

ADDRESS

BY

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PRESIDENT.

ONCE more has the Dominion of Canada invited the British Association for the Advancement of Science to hold one of the annual meetings of its members within the Canadian territory ; and for a second time has the Association had the honour and pleasure of accepting the proffered hospitality.

In doing so, the Association has felt that if by any possibility the scientific welfare of a locality is promoted by its being the scene of such a meeting, the claims should be fully recognised of those who, though not dwelling in the British Isles, are still inhabitants of that Greater Britain whose prosperity is so intimately connected with the fortunes of the Mother Country.

Here, especially, as loyal subjects of one beloved Sovereign, the sixtieth year of whose beneficent reign has just been celebrated with equal rejoicing in all parts of her Empire ; as speaking the same tongue, and as in most instances connected by the ties of one common parentage, we are bound together in all that can promote our common interests.

There is, in all probability, nothing that will tend more to advance those interests than the diffusion of science in all parts of the British Empire, and it is towards this end that the aspirations of the British Association are ever directed, even if in many instances the aim may not be attained.

We are, as already mentioned, indebted to Canada for previous hospitality, but we must also remember that, since the time when we last assembled on this side of the Atlantic, the Dominion has provided the

Association with a President, Sir William Dawson, whose name is alike well known in Britain and America, and whose reputation is indeed world-wide. We rejoice that we have still among us the pioneer of American geology, who among other discoveries first made us acquainted with the 'Air-breathers of the Coal,' the terrestrial or more properly arboreal Saurians of the New Brunswick and Nova Scotia Coal-measures.

On our last visit to Canada, in 1884, our place of assembly was Montreal, a city which is justly proud of her McGill University; to-day we meet within the buildings of another of the Universities of this vast Dominion—and in a city, the absolute fitness of which for such a purpose must have been foreseen by the native Indian tribes when they gave to a small aggregation of huts upon this spot the name of Toronto—'the place of meetings.'

Our gathering this year presents a feature of entire novelty and extreme interest, inasmuch as the sister Association of the United States of America,—still mourning the loss of her illustrious President, Professor Cope,—and some other learned societies, have made special arrangements to allow of their members coming here to join us. I need hardly say how welcome their presence is, nor how gladly we look forward to their taking part in our discussions, and aiding us by interchange of thought. To such a meeting the term 'international' seems almost misapplied. It may rather be described as a family gathering, in which our relatives more or less distant in blood, but still intimately connected with us by language, literature, and habits of thought, have spontaneously arranged to take part.

The domain of science is no doubt one in which the various nations of the civilised world meet upon equal terms, and for which no other passport is required than some evidence of having striven towards the advancement of natural knowledge. Here, on the frontier between the two great English-speaking nations of the world, who is there that does not inwardly feel that anything which conduces to an intimacy between the representatives of two countries, both of them actively engaged in the pursuit of science, may also, through such an intimacy, react on the affairs of daily life, and aid in preserving those cordial relations that have now for so many years existed between the great American Republic and the British Islands, with which her early foundations are indissolubly connected? The present year has witnessed an interchange of courtesies which has excited the warmest feelings of approbation on both sides of the Atlantic. I mean the return to its proper custodians of one of the most interesting of the relics of the Pilgrim Fathers, the Log of the 'Mayflower.' May this return, trifling in itself, be of happy augury as testifying to the feelings of mutual regard and esteem which animate the hearts both of the donors and of the recipients!

At our meeting in Montreal the President was an investigator who had already attained to a foremost place in the domains of Physics and

Mathematics, Lord Rayleigh. In his address he dealt mainly with topics, such as Light, Heat, Sound, and Electricity, on which he is one of our principal authorities. His name and that of his fellow-worker, Professor Ramsay, are now and will in all future ages be associated with the discovery of the new element, Argon. Of the ingenious methods by which that discovery was made, and the existence of Argon established, this is not the place to speak. One can only hope that the element will not always continue to justify its name by its inertness.

The claims of such a leader in physical science as Lord Rayleigh to occupy the Presidential chair are self-evident, but possibly those of his successor on this side of the Atlantic are not so immediately apparent. I cannot for a moment pretend to place myself on the same purely scientific level as my distinguished friend and for many years colleague, Lord Rayleigh, and my claims, such as they are, seem to me to rest on entirely different grounds.

Whatever little I may have indirectly been able to do in assisting to promote the advancement of science, my principal efforts have now for many years been directed towards attempting to forge those links in the history of the world, and especially of humanity, that connect the past with the present, and towards tracing that course of evolution which plays as important a part in the physical and moral development of man as it does in that of the animal and vegetable creation.

It appears to me, therefore, that my election to this important post may, in the main, be regarded as a recognition by this Association of the value of Archæology as a science.

Leaving all personal considerations out of question, I gladly hail this recognition, which is, indeed, in full accordance with the attitude already for many years adopted by the Association towards Anthropology, one of the most important branches of true Archæology.

It is no doubt hard to define the exact limits which are to be assigned to Archæology as a science, and Archæology as a branch of History and Belles Lettres. A distinction is frequently drawn between science on the one hand, and knowledge or learning on the other ; but translate the terms into Latin, and the distinction at once disappears. In illustration of this I need only cite Bacon's great work on the 'Advancement of Learning,' which was, with his own aid, translated into Latin under the title '*De Augmentis Scientiarum.*'

It must, however, be acknowledged that a distinction does exist between Archæology proper, and what, for want of a better word, may be termed Antiquarianism. It may be interesting to know the internal arrangements of a Dominican convent in the middle ages ; to distinguish between the different mouldings characteristic of the principal styles of Gothic architecture ; to determine whether an English coin bearing the name of Henry was struck under Henry II., Richard, John, or Henry III., or to decide whether some given edifice was erected in Roman,

Saxon, or Norman times. But the power to do this, though involving no small degree of detailed knowledge and some acquaintance with scientific methods, can hardly entitle its possessors to be enrolled among the votaries of science.

A familiarity with all the details of Greek and Roman mythology and culture must be regarded as a literary rather than a scientific qualification ; and yet when among the records of classical times we come upon traces of manners and customs which have survived for generations, and which seem to throw some rays of light upon the dim past, when history and writing were unknown, we are, I think, approaching the boundaries of scientific Archæology.

Every reader of Virgil knows that the Greeks were not merely orators, but that with a pair of compasses they could describe the movements of the heavens and fix the rising of the stars ; but when by modern Astronomy we can determine the heliacal rising of some well-known star, with which the worship in some given ancient temple is known to have been connected, and can fix its position on the horizon at some particular spot, say, three thousand years ago, and then find that the axis of the temple is directed exactly towards that spot, we have some trustworthy scientific evidence that the temple in question must have been erected at a date approximately 1100 years B.C. If on or close to the same site we find that more than one temple was erected, each having a different orientation, these variations, following as they may fairly be presumed to do the changing position of the rising of the dominant star, will also afford a guide as to the chronological order of the different foundations. The researches of Mr. Penrose seem to show that in certain Greek temples, of which the date of foundation is known from history, the actual orientation corresponds with that theoretically deduced from astronomical data.

Sir J. Norman Lockyer has shown that what holds good for Greek temples applies to many of far earlier date in Egypt, though up to the present time hardly a sufficient number of accurate observations have been made to justify us in foreseeing all the instructive results that may be expected to arise from Astronomy coming to the aid of Archæology.

The intimate connection of Archæology with other sciences is in no case so evident as with respect to Geology, for when considering subjects such as those I shall presently discuss, it is almost impossible to say where the one science ends and the other begins.

By the application of geological methods many archæological questions relating even to subjects on the borders of the historical period have been satisfactorily solved. A careful examination of the limits of the area over which its smaller coins are found has led to the position of many an ancient Greek city being accurately ascertained ; while in England it has only been by treating the coins of the Ancient Britons, belonging to a period before the Roman occupation, as if they were actual fossils, that the territories under the dominion of the various kings and princes who struck them have been approximately determined. In arranging the

chronological sequence of these coins, the evolution of their types—a process almost as remarkable, and certainly as well-defined, as any to be found in nature—has served as an efficient guide. I may venture to add that the results obtained from the study of the morphology of this series of coins were published ten years before the appearance of Darwin's great work on the 'Origin of Species.'

When we come to the consideration of the relics of the Early Iron and Bronze Ages, the aid of Chemistry has of necessity to be invoked. By its means we are able to determine whether the iron of a tool or weapon is of meteoritic or volcanic origin, or has been reduced from iron-ore, in which case considerable knowledge of metallurgy would be involved on the part of those who made it. With bronze antiquities the nature and extent of the alloys combined with the copper may throw light not only on their chronological position, but on the sources whence the copper, tin, and other metals of which they consist were originally derived. I am not aware of there being sufficient differences in the analyses of the native copper from different localities in the region in which we are assembled, for Canadian Archæologists to fix the sources from which the metal was obtained which was used in the manufacture of the ancient tools and weapons of copper that are occasionally discovered in this part of the globe.

Like Chemistry, Mineralogy and Petrology may be called to the assistance of Archæology in determining the nature and source of the rocks of which ancient stone implements are made; and, thanks to researches of the followers of those sciences, the old view that all such implements formed of jade and found in Europe must of necessity have been fashioned from material imported from Asia can no longer be maintained. In one respect the Archæologist differs in opinion from the Mineralogist—namely, as to the propriety of chipping off fragments from perfect and highly finished specimens for the purpose of submitting them to microscopic examination.

I have hitherto been speaking of the aid that other sciences can afford to Archæology when dealing with questions that come almost, if not quite, within the fringe of history, and belong to times when the surface of our earth presented much the same configuration as regards the distribution of land and water, and hill and valley, as it does at present, and when, in all probability, the climate was much the same as it now is. When, however, we come to discuss that remote age in which we find the earliest traces that are at present known of Man's appearance upon earth, the aid of Geology and Palæontology becomes absolutely imperative.

The changes in the surface configuration and in the extent of the land, especially in a country like Britain, as well as the modifications of the fauna and flora since those days, have been such that the Archæologist pure and simple is incompetent to deal with them, and he must either himself undertake the study of these other sciences or call experts in them

to his assistance. The evidence that Man had already appeared upon the earth is afforded by stone implements wrought by his hands, and it falls strictly within the province of the Archæologist to judge whether given specimens were so wrought or not ; it rests with the Geologist to determine their stratigraphical or chronological position, while the Palæontologist can pronounce upon the age and character of the associated fauna and flora.

If left to himself the Archæologist seems too prone to build up theories founded upon form alone, irrespective of geological conditions. The Geologist, unaccustomed to archæological details, may readily fail to see the difference between the results of the operations of Nature and those of Art, and may be liable to trace the effects of man's handiwork in the chipping, bruising, and wearing which in all ages result from natural forces ; but the united labours of the two, checked by those of the Palæontologist, cannot do otherwise than lead towards sound conclusions.

It will perhaps be expected of me that I should on the present occasion bring under review the state of our present knowledge with regard to the Antiquity of Man ; and probably no fitter place could be found for the discussion of such a topic than the adopted home of my venerated friend, the late Sir Daniel Wilson, who first introduced the word 'pre-historic' into the English language.

Some among us may be able to call to mind the excitement, not only among men of science but among the general public, when, in 1859, the discoveries of M. Boucher de Perthes and Dr. Rigollot in the gravels of the valley of the Somme, at Abbeville and Amiens, were confirmed by the investigations of the late Sir Joseph Prestwich, myself, and others, and the co-existence of Man with the extinct animals of the Quaternary fauna, such as the mammoth and woolly-haired rhinoceros, was first virtually established. It was at the same time pointed out that these relics belonged to a far earlier date than the ordinary stone weapons found upon the surface, which usually showed signs of grinding or polishing, and that in fact there were two Stone Ages in Britain. To these the terms Neolithic and Palæolithic were subsequently applied by Sir John Lubbock.

The excitement was not less, when, at the meeting of this Association at Aberdeen in the autumn of that year, Sir Charles Lyell, in the presence of the Prince Consort, called attention to the discoveries in the valley of the Somme, the site of which he had himself visited, and to the vast lapse of time indicated by the position of the implements in drift-deposits a hundred feet above the existing river.

The conclusions forced upon those who examined the facts on the spot did not receive immediate acceptance by all who were interested in Geology and Archæology, and fierce were the controversies on the subject that were carried on both in the newspapers and before various learned societies.

It is at the same time instructive and amusing to look back on the discussions of those days. While one class of objectors accounted for the configuration of the flint implements from the gravels by some unknown chemical agency, by the violent and continued gyratory action of water, by fracture resulting from pressure, by rapid cooling when hot or by rapid heating when cold, or even regarded them as aberrant forms of fossil fishes, there were others who, when compelled to acknowledge that the implements were the work of men's hands, attempted to impugn and set aside the evidence as to the circumstances under which they had been discovered. In doing this they adopted the view that the worked flints had either been introduced into the containing beds at a comparatively recent date, or if they actually formed constituent parts of the gravel then that this was a mere modern alluvium resulting from floods at no very remote period.

In the course of a few years the main stream of scientific thought left this controversy behind, though a tendency to cut down the lapse of time necessary for all the changes that have taken place in the configuration of the surface of the earth and in the character of its occupants since the time of the Palæolithic gravels, still survives in the inmost recesses of the hearts of not a few observers.

In his Address to this Association at the Bath meeting of 1864, Sir Charles Lyell struck so true a note that I am tempted to reproduce the paragraph to which I refer :—

‘When speculations on the long series of events which occurred in the glacial and post-glacial periods are indulged in, the imagination is apt to take alarm at the immensity of the time required to interpret the monuments of these ages, all referable to the era of existing species. In order to abridge the number of centuries which would otherwise be indispensable, a disposition is shown by many to magnify the rate of change in pre-historic times by investing the causes which have modified the animate and inanimate world with extraordinary and excessive energy. It is related of a great Irish orator of our day that when he was about to contribute somewhat parsimoniously towards a public charity, he was persuaded by a friend to make a more liberal donation. In doing so he apologized for his first apparent want of generosity by saying that his early life had been a constant struggle with scanty means, and that “they who are born to affluence cannot easily imagine how long a time it takes to get the chill of poverty out of one's bones.” In like manner we of the living generation, when called upon to make grants of thousands of centuries in order to explain the events of what is called the modern period, shrink naturally at first from making what seems so lavish an expenditure of past time. Throughout our early education we have been accustomed to such strict economy in all that relates to the chronology of the earth and its inhabitants in remote ages, so fettered have we been by old traditional beliefs, that even when our reason is convinced, and we

are persuaded that we ought to make more liberal grants of time to the Geologist, we feel how hard it is to get the chill of poverty out of our bones.'

Many, however, have at the present day got over this feeling, and of late years the general tendency of those engaged upon the question of the antiquity of the human race has been in the direction of seeking for evidence by which the existence of Man upon the earth could be carried back to a date earlier than that of the Quaternary gravels.

There is little doubt that such evidence will eventually be forthcoming, but, judging from all probability, it is not in Northern Europe that the cradle of the human race will eventually be discovered, but in some part of the world more favoured by a tropical climate, where abundant means of subsistence could be procured, and where the necessity for warm clothing did not exist.

Before entering into speculations on this subject, or attempting to lay down the limits within which we may safely accept recent discoveries as firmly established, it will be well to glance at some of the cases in which implements are stated to have been found under circumstances which raise a presumption of the existence of man in pre-Glacial, Pliocene, or even Miocene times.

Flint implements of ordinary Palæolithic type have, for instance, been recorded as found in the Eastern Counties of England, in beds beneath the Chalky Boulder Clay; but on careful examination the geological evidence has not to my mind proved satisfactory, nor has it, I believe, been generally accepted. Moreover, the archaeological difficulty that Man, at two such remote epochs as the pre-Glacial and the post-Glacial, even if the term Glacial be limited to the Chalky Boulder Clay, should have manufactured implements so identical in character that they cannot be distinguished apart, seems to have been entirely ignored.

Within the last few months we have had the report of worked flints having been discovered in the late Pliocene Forest Bed of Norfolk, but in that instance the signs of human workmanship upon the flints are by no means apparent to all observers.

But such an antiquity as that of the Forest Bed is as nothing when compared with that which would be implied by the discoveries of the work of men's hands in the Pliocene and Miocene beds of England, France, Italy, and Portugal, which have been accepted by some Geologists. There is one feature in these cases which has hardly received due attention, and that is the isolated character of the reputed discoveries. Had man, for instance, been present in Britain during the Crag Period, it would be strange indeed if the sole traces of his existence that he left were a perforated tooth of a large shark, the sawn rib of a manatee, and a beaming full face, carved on the shell of a pectunculus!

In an address to the Anthropological Section at the Leeds meeting of this Association in 1890 I dealt somewhat fully with these supposed

discoveries of the remains of human art in beds of Tertiary date ; and I need not here go further into the question. Suffice it to say that I see no reason why the verdict of ' not proven ' at which I then arrived should be reversed.

In the case of a more recent discovery in Upper Burma in beds at first pronounced to be Upper Miocene, but subsequently ' definitely ascertained to be Pliocene,' some of the flints are of purely natural and not artificial origin, so that two questions arise : first, Were the fossil remains associated with the worked flints or with those of natural forms ? And second, Were they actually found in the bed to which they have been assigned, or did they merely lie together on the surface ?

Even the *Pithecanthropus erectus* of Dr. Eugène Dubois from Java meets with some incredulous objectors from both the physiological and the geological sides. From the point of view of the latter the difficulty lies in determining the exact age of what are apparently alluvial beds in the bottom of a river valley.

When we return to Palæolithic man, it is satisfactory to feel that we are treading on comparatively secure ground, and that the discoveries of the last forty years in Britain alone enable us to a great extent to reconstitute his history. We may not know the exact geological period when first he settled in the British area, but we have good evidence that he occupied it at a time when the configuration of the surface was entirely different from what it is at present : when the river valleys had not been cut down to anything like their existing depth, when the fauna of the country was of a totally different character from that of the present day, when the extension of the southern part of the island seaward was in places such that the land was continuous with that of the continent, and when in all probability a far more rainy climate prevailed. We have proofs of the occupation of the country by man during the long lapse of time that was necessary for the excavation of the river valleys. We have found the old floors on which his habitations were fixed, we have been able to trace him at work on the manufacture of flint instruments, and by building up the one upon the other the flakes struck off by the primæval workman in those remote times we have been able to reconstruct the blocks of flint which served as his material.

That the duration of the Palæolithic Period must have extended over an almost incredible length of time is sufficiently proved by the fact that valleys, some miles in width and of a depth of from 100 to 150 feet, have been eroded since the deposit of the earliest implement-bearing beds. Nor is the apparent duration of this period diminished by the consideration that the floods which hollowed out the valleys were not in all probability of such frequent occurrence as to teach Palæolithic man by experience the danger of settling too near to the streams, for had he kept to the higher slopes of the valley there would have been but little chance of his implements having so constantly formed constituent parts of the gravels deposited by the floods.

The examination of British cave-deposits affords corroborative evidence of this extended duration of the Palæolithic Period. In Kent's Cavern at Torquay, for instance, we find in the lowest deposit, the breccia below the red cave-earth, implements of flint and chert corresponding in all respects with those of the high level and most ancient river gravels. In the cave-earth these are scarcer, though implements occur which also have their analogues in the river deposits ; but, what is more remarkable, harpoons of reindeer's horn and needles of bone are present, identical in form and character with those of the caverns of the Reindeer Period in the South of France, and suggestive of some bond of union or identity of descent between the early troglodytes, whose habitations were geographically so widely separated the one from the other.

In a cavern at Creswell Crags, on the confines of Derbyshire and Nottinghamshire, a bone has moreover been found engraved with a representation of parts of a horse in precisely the same style as the engraved bones of the French caves.

It is uncertain whether any of the River-drift specimens belong to so late a date as these artistic cavern-remains ; but the greatly superior antiquity of even these to any Neolithic relics is testified by the thick layer of stalagmite, which had been deposited in Kent's Cavern before its occupation by men of the Neolithic and Bronze Periods.

Towards the close of the period covered by the human occupation of the French caves, there seems to have been a dwindling in the number of the larger animals constituting the Quaternary fauna, whereas their remains are present in abundance in the lower and therefore more recent of the valley gravels. This circumstance may afford an argument in favour of regarding the period represented by the later French caves as a continuation of that during which the old river gravels were deposited, and yet the great change in the fauna that has taken place since the latest of the cave-deposits included in the Palæolithic Period is indicative of an immense lapse of time.

How much greater must have been the time required for the more conspicuous change between the old Quaternary fauna of the river gravels and that characteristic of the Neolithic Period !

As has been pointed out by Prof. Boyd Dawkins, only thirty-one out of the forty-eight well-ascertained species living in the post-Glacial or River-drift Period survived into pre-historic or Neolithic times. We have not, indeed, any means at command for estimating the number of centuries which such an important change indicates ; but when we remember that the date of the commencement of the Neolithic or Surface Stone Period is still shrouded in the mist of a dim antiquity, and that prior to that commencement the River-drift Period had long come to an end ; and when we further take into account the almost inconceivable ages that even under the most favourable conditions the excavation of wide and deep valleys by river action implies, the remoteness of the date

at which the Palæolithic Period had its beginning almost transcends our powers of imagination.

We find distinct traces of river action from 100 to 200 feet above the level of existing streams and rivers, and sometimes at a great distance from them ; we observe old fresh-water deposits on the slopes of valleys several miles in width ; we find that long and lofty escarpments of rock have receded unknown distances since their summits were first occupied by Palæolithic man ; we see that the whole side of a wide river valley has been carried away by an invasion of the sea, which attacked and removed a barrier of chalk cliffs from 400 to 600 feet in height ; we find that what was formerly an inland river has been widened out into an arm of the sea, now the highway of our fleets, and that gravels which were originally deposited in the bed of some ancient river now cap isolated and lofty hills.

And yet, remote as the date of the first known occupation of Britain by man may be, it belongs to what, geologically speaking, must be regarded as a quite recent period, for we are now in a position to fix with some degree of accuracy its place on the geological scale. Thanks to investigations ably carried out at Hoxne in Suffolk, and at Hitchin in Hertfordshire, by Mr. Clement Reid, under the auspices of this Association and of the Royal Society, we know that the implement-bearing beds at those places undoubtedly belong to a time subsequent to the deposit of the Great Chalky Boulder Clay of the Eastern Counties of England. It is, of course, self-evident that this vast deposit, in whatever manner it may have been formed, could not, for centuries after its deposition was complete, have presented a surface inhabitable by man. Moreover, at a distance but little farther north, beds exist which also, though at a somewhat later date, were apparently formed under Glacial conditions. At Hoxne the interval between the deposit of the Boulder Clay and of the implement-bearing beds is distinctly proved to have witnessed at least two noteworthy changes in climate. The beds immediately reposing on the Clay are characterised by the presence of alder in abundance, of hazel, and yew, as well as by that of numerous flowering plants indicative of a temperate climate very different from that under which the Boulder Clay itself was formed. Above these beds characterised by temperate plants, comes a thick and more recent series of strata, in which leaves of the dwarf Arctic willow and birch abound, and which were in all probability deposited under conditions like those of the cold regions of Siberia and North America.

At a higher level and of more recent date than these—from which they are entirely distinct—are the beds containing Palæolithic implements, formed in all probability under conditions not essentially different from those of the present day. However this may be, we have now conclusive evidence that the Palæolithic implements are, in the Eastern Counties of England, of a date long posterior to that of the Great Chalky Boulder Clay.

It may be said, and said truly, that the implements at Hoxne cannot be shown to belong to the beginning rather than to some later stage of the Palæolithic Period. The changes, however, that have taken place at Hoxne in the surface configuration of the country prove that the beds containing the implements cannot belong to the close of that period.

It must, moreover, be remembered that in what are probably the earliest of the Palæolithic deposits of the Eastern Counties, those at the highest level, near Brandon in Norfolk, where the gravels contain the largest proportion of pebbles derived from Glacial beds, some of the implements themselves have been manufactured from materials not native to the spot but brought from a distance, and derived in all probability either from the Boulder Clay or from some of the beds associated with it.

We must, however, take a wider view of the whole question, for it must not for a moment be supposed that there are the slightest grounds for believing that the civilisation, such as it was, of the Palæolithic Period originated in the British Isles. We find in other countries implements so identical in form and character with British specimens that they might have been manufactured by the same hands. These occur over large areas in France under similar conditions to those that prevail in England. The same forms have been discovered in the ancient river gravels of Italy, Spain, and Portugal. Some few have been recorded from the north of Africa, and analogous types occur in considerable numbers in the south of that continent. On the banks of the Nile, many hundreds of feet above its present level, implements of the European types have been discovered; while in Somaliland, in an ancient river valley at a great elevation above the sea, Mr. Seton-Karr has collected a large number of implements formed of flint and quartzite, which, judging from their form and character, might have been dug out of the drift deposits of the Somme or the Seine, the Thames or the ancient Solent.

In the valley of the Euphrates implements of the same kind have also been found, and again farther east in the lateritic deposits of Southern India they have been obtained in considerable numbers. It is not a little remarkable, and is at the same time highly suggestive, that a form of implement almost peculiar to Madras reappears among implements from the very ancient gravels of the Manzanares at Madrid. In the case of the African discoveries we have as yet no definite Palæontological evidence by which to fix their antiquity, but in the Narbadá Valley of Western India Palæolithic implements of quartzite seem to be associated with a local fauna of Pleistocene age, comprising, like that of Europe, the elephant, hippopotamus, ox, and other mammals of species now extinct. A correlation of the two faunas with a view of ascertaining their chronological relations is beset with many difficulties, but there seems reason for accepting this Indian Pleistocene fauna as in some degree more ancient than the European.

Is this not a case in which the imagination may be fairly invoked in aid of science? May we not from these data attempt in some degree to build up and reconstruct the early history of the human family? There, in Eastern Asia, in a tropical climate, with the means of subsistence readily at hand, may we not picture to ourselves our earliest ancestors gradually developing from a lowly origin, acquiring a taste for hunting, if not indeed being driven to protect themselves from the beasts around them, and evolving the more complicated forms of tools or weapons from the simpler flakes which had previously served them as knives? May we not imagine that, when once the stage of civilisation denoted by these Palæolithic implements had been reached, the game for the hunter became scarcer, and that his life in consequence assumed a more nomad character? Then, and possibly not till then, may a series of migrations to 'fresh woods and pastures new' not unnaturally have ensued, and these following the usual course of 'westward towards the setting sun' might eventually lead to a Palæolithic population finding its way to the extreme borders of Western Europe, where we find such numerous traces of its presence.

How long a term of years may be involved in such a migration it is impossible to say, but that such a migration took place the phenomena seem to justify us in believing. It can hardly be supposed that the process that I have shadowed forth was reversed, and that Man, having originated in North-Western Europe, in a cold climate where clothing was necessary and food scarce, subsequently migrated eastward to India and southward to the Cape of Good Hope! As yet, our records of discoveries in India and Eastern Asia are but scanty; but it is there that the traces of the cradle of the human race are, in my opinion, to be sought, and possibly future discoveries may place upon a more solid foundation the visionary structure that I have ventured to erect.

It may be thought that my hypothesis does not do justice to what Sir Thomas Browne has so happily termed 'that great antiquity, America.' I am, however, not here immediately concerned with the important Neolithic remains of all kinds with which this great continent abounds. I am now confining myself to the question of Palæolithic man and his origin, and in considering it I am not unmindful of the Trenton implements, though I must content myself by saying that the 'turtle-back' form is essentially different from the majority of those on the wide dissemination of which I have been speculating, and, moreover, as many here present are aware, the circumstances of the finding of these American implements are still under careful discussion.

Leaving them out of the question for the present, it may be thought worth while to carry our speculations rather further, and to consider the relations in time between the Palæolithic and the Neolithic Periods. We have seen that the stage in human civilisation denoted by the use of the ordinary forms of Palæolithic implements must have extended over a vast

period of time if we have to allow for the migration of the primæval hunters from their original home, wherever it may have been in Asia or Africa, to the west of Europe, including Britain. We have seen that, during this migration, the forms of the weapons and tools made from silicious stones had become, as it were, stereotyped, and further, that, during the subsequent extended period implied by the erosion of the valleys, the modifications in the form of the implements and the changes in the fauna associated with the men who used them were but slight.

At the close of the period during which the valleys were being eroded comes that represented by the latest occupation of the caves by Palæolithic man, when both in Britain and in the south of France the reindeer was abundant; but among the stone weapons and implements of that long troglodytic phase of man's history not a single example with the edge sharpened by grinding has as yet been found. All that can safely be said is that the larger implements as well as the larger mammals had become scarcer, that greater power in chipping flint had been attained, that the arts of the engraver and the sculptor had considerably developed, and that the use of the bow had probably been discovered.

Directly we encounter the relics of the Neolithic Period, often, in the case of the caves lately mentioned, separated from the earlier remains by a thick layer of underlying stalagmite, we find flint hatchets polished at the edge and on the surface, cutting at the broad and not at the narrow end, and other forms of implements associated with a fauna in all essential respects identical with that of the present day.

Were the makers of these polished weapons the direct descendants of Palæolithic ancestors whose occupation of the country was continuous from the days of the old river gravels? or had these long since died out, so that after Western Europe had for ages remained uninhabited, it was re-peopled in Neolithic times by the immigration of some new race of men? Was there, in fact, a 'great gulf fixed' between the two occupations? or was there in Europe a gradual transition from the one stage of culture to the other?

It has been said that 'what song the Syrens sang, or what name Achilles assumed when he hid himself among women, though puzzling questions, are not beyond all conjecture'; and though the questions now proposed may come under the same category, and must await the discovery of many more essential facts before they receive definite and satisfactory answers, we may, I think, throw some light upon them if we venture to take a few steps upon the seductive if insecure paths of conjecture. So far as I know we have as yet no trustworthy evidence of any transition from the one age to the other, and the gulf between them remains practically unbridged. We can, indeed, hardly name the part of the world in which to seek for the cradle of Neolithic civilisation, though we know that traces of what appear to have been a stone-using people have been discovered in Egypt, and that what must be among the latest

of the relics of their industry have been assigned to a date some 3,500 to 4,000 years before our era. The men of that time had attained to the highest degree of skill in working flint that has ever been reached. Their beautifully made knives and spear-heads seem indicative of a culminating point reached after long ages of experience ; but whence these artists in flint came or who they were is at present absolutely unknown, and their handiworks afford no clue to help us in tracing their origin.

Taking a wider survey, we may say that, generally speaking, not only the fauna but the surface configuration of the country were, in Western Europe at all events, much the same at the commencement of the Neolithic Period as they are at the present day. We have, too, no geological indications to aid us in forming any chronological scale.

The occupation of some of the caves in the south of France seems to have been carried on after the erosion of the neighbouring river valleys had ceased, and so far as our knowledge goes these caves offer evidence of being the latest in time of those occupied by Man during the Palæolithic Period. It seems barely possible that, though in the north of Europe there are no distinct signs of such late occupation, yet that, in the south, Man may have lived on, though in diminished numbers ; and that in some of the caves, such, for instance, as those in the neighbourhood of Mentone, there may be traces of his existence during the transitional period that connects the Palæolithic and Neolithic Ages. If this were really the case, we might expect to find some traces of a dissemination of Neolithic culture from a North Italian centre, but I much doubt whether any such traces actually exist.

If it had been in that part of the world that the transition took place, how are we to account for the abundance of polished stone hatchets found in Central India ? Did Neolithic man return eastward by the same route as that by which in remote ages his Palæolithic predecessor had migrated westward ? Would it not be in defiance of all probability to answer such a question in the affirmative ? We have, it must be confessed, nothing of a substantial character to guide us in these speculations ; but, pending the advent of evidence to the contrary, we may, I think, provisionally adopt the view that owing to failure of food, climatal changes, or other causes, the occupation of Western Europe by Palæolithic man absolutely ceased, and that it was not until after an interval of long duration that Europe was re-peopled by a race of men immigrating from some other part of the globe where the human race had survived, and in course of ages had developed a higher stage of culture than that of Palæolithic man.

I have been carried away by the liberty allowed for conjecture into the regions of pure imagination, and must now return to the realms of fact, and one fact on which I desire for a short time to insist is that of the existence at the present day, in close juxtaposition with our own civilisation, of races of men who, at all events but a few generations ago,

lived under much the same conditions as did our own Neolithic predecessors in Europe.

The manners and customs of these primitive tribes and peoples are changing day by day, their languages are becoming obsolete, their myths and traditions are dying out, their ancient processes of manufacture are falling into oblivion, and their numbers are rapidly diminishing, so that it seems inevitable that ere long many of these interesting populations will become absolutely extinct. The admirable Bureau of Ethnology instituted by our neighbours in the United States of America has done much towards preserving a knowledge of the various native races in this vast continent ; and here in Canada the annual Archæological Reports presented to the Minister of Education are rendering good service in the same cause.

Moreover the Committee of this Association appointed to investigate the physical characters, languages, and industrial and social conditions of the North-Western tribes of the Dominion of Canada is about to present its twelfth and final report, which in conjunction with those already presented will do much towards preserving a knowledge of the habits and languages of those tribes. It is sad to think that Mr. Horatio Hale, whose comprehensive grasp of the bearings of ethnological questions, and whose unremitting labours have so materially conduced to the success of the Committee, should be no longer among us. Although this report is said to be final, it is to be hoped that the Committee may be able to indicate lines upon which future work in the direction of ethnological and archæological research may be profitably carried on in this part of Her Majesty's dominions.

It is, however, lamentable to notice how little is being or has been officially done towards preserving a full record of the habits, beliefs, arts, myths, languages, and physical characteristics of the countless other tribes and nations more or less uncivilised which are comprised within the limits of the British Empire. At the meeting of this Association held last year at Liverpool it was resolved by the General Committee 'that it is of urgent importance to press upon the Government the necessity of establishing a Bureau of Ethnology for Greater Britain, which by collecting information with regard to the native races within and on the borders of the Empire will prove of immense value to science and to the Government itself.' It has been suggested that such a bureau might with the greatest advantage and with the least outlay and permanent expense be connected either with the British Museum or with the Imperial Institute, and the project has already been submitted for the consideration of the Trustees of the former establishment.

The existence of an almost unrivalled ethnological collection in the Museum, and the presence there of officers already well versed in ethnological research, seem to afford an argument in favour of the proposed bureau being connected with it. On the other hand, the Imperial Insti-

tute was founded with an especial view to its being a centre around which every interest connected with the dependencies of the Empire might gather for information and support. The establishment within the last twelve months of a Scientific Department within the Institute, with well-appointed laboratories and a highly trained staff, shows how ready are those concerned in its management to undertake any duties that may conduce to the welfare of the outlying parts of the British Empire ; a fact of which I believe that Canada is fully aware. The Institute is therefore likely to develop, so far as its scientific department is concerned, into a Bureau of advice in all matters scientific and technical, and certainly a Bureau of Ethnology such as that suggested would not be out of place within its walls.

Wherever such an institution is to be established, the question of its existence must of necessity rest with Her Majesty's Government and Treasury, inasmuch as without funds, however moderate, the undertaking cannot be carried on. I trust that in considering the question it will always be borne in mind that in the relations between civilised and uncivilised nations and races it is of the first importance that the prejudices and especially the religious or semi-religious and caste prejudices of the latter should be thoroughly well known to the former. If but a single 'little war' could be avoided in consequence of the knowledge acquired and stored up by the Bureau of Ethnology preventing such a misunderstanding as might culminate in warfare, the cost of such an institution would quickly be saved.

I fear that it will be thought that I have dwelt too long on primæval man and his modern representatives, and that I should have taken this opportunity to discuss some more general subject, such as the advances made in the various departments of science since last this Association met in Canada. Such a subject would no doubt have afforded an infinity of interesting topics on which to dilate. Spectrum analysis, the origin and nature of celestial bodies, photography, the connection between heat, light, and electricity, the practical applications of the latter, terrestrial magnetism, the liquefaction and solidification of gases, the behaviour of elements and compounds under the influence of extreme cold, the nature and uses of the Röntgen rays, the advances in bacteriology and in prophylactic medicine, might all have been passed under review, and to many of my audience would have seemed to possess greater claims to attention than the subject that I have chosen.

It must, however, be borne in mind that most, if not indeed all, of these topics will be discussed by more competent authorities in the various Sections of the Association by means of the Presidential addresses or otherwise. Nor must it be forgotten that I occupy this position as a representative of Archæology, and am therefore justified in bringing before you a subject in which every member of every race of mankind ought to be interested—the antiquity of the human family and the scenes of its infancy.

Others will direct our thoughts in other directions, but the farther we proceed the more clearly shall we realise the connection and interdependence of all departments of science. Year after year, as meetings of this Association take place, we may also foresee that ‘many shall run to and fro and knowledge shall be increased.’ Year after year advances will be made in science, and in reading that Book of Nature that lies ever open before our eyes ; successive stones will be brought for building up that Temple of Knowledge of which our fathers and we have laboured to lay the foundations. May we not well exclaim with old Robert Recorde ?—

‘Oh woorthy temple of Goddes magnificence : Oh throne of glorye and seate of the lorde : thy substance most pure what tonge can describe ? thy signes are so wonderous, surmountinge mannes witte, the effects of thy motions so diuers in kinde : so harde for to searche, and worse for to fynde—Thy woorkes are all wonderous, thy cunning unknowen : yet seedes of all knowledge in that booke are sowed—And yet in that booke who rightly can reade, to all secrete knowledge it will him straighte leade.’¹

¹ Preface to Robert Recorde’s *Castle of Knowledge*, 1556.

British Association for the Advancement of Science.

DOVER, 1899.

ADDRESS

BY

PROFESSOR SIR MICHAEL FOSTER, K.C.B., SEC.R.S.
PRESIDENT.

HE who until a few minutes ago was your President said somewhere at the meeting at Bristol, and said with truth, that among the qualifications needed for the high honour of Presidency of the British Association for the Advancement of Science, that of being old was becoming more and more dominant. He who is now attempting to speak to you feels that he is rapidly earning that distinction. But the Association itself is older than its President ; it has seen pass away the men who, wise in their generation, met at York on September 27, 1831, to found it ; it has seen other great men who in bygone years served it as Presidents, or otherwise helped it on, sink one after another into the grave. Each year, indeed, when it plants its flag as a signal of its yearly meeting, that flag floats half-mast high in token of the great losses which the passing year has brought. This year is no exception ; the losses, indeed, are perhaps unwontedly heavy. I will not attempt to call over the sad roll-call ; but I must say a word about one who was above most others a faithful and zealous friend of the Association. Sir Douglas Galton joined the Association in 1860. From 1871 to 1895, as one of the General Secretaries, he bore, and bore to the great good of the Association, a large share of the burden of the Association's work. How great that share was is perhaps especially known to the many men, among whom I am proud to count myself, who during his long term of office served in succession with him as brother General Secretary. In 1895, at Ipswich, he left the post of General Secretary, but only to become President. So long and so constantly did he labour for the good of the Association that he seemed to be an integral part of it, and meeting as we do to-day, and as we henceforward must do, without Douglas Galton, we feel something greatly missing. This year, perhaps even more than in other years, we could have wished him to be among us ; for to-day the Association may look with joy, not unmixed with pride, on the realisation of a project in forwarding which it has had a conspicuous share, on the

commencement of an undertaking which is not only a great thing in itself, but which, we trust, is the beginning of still greater things to come. And the share which the Association has had in this was largely Sir Douglas Galton's doing. In his Address as President of Section A, at the meeting of the Association at Cardiff in 1891, Professor Oliver Lodge expounded with pregnant words how urgently, not pure science only, but industry and the constructive arts—for the interests of these are ever at bottom the same—needed the aid of some national establishment for the prosecution of prolonged and costly physical researches, which private enterprise could carry out in a lame fashion only, if at all. Lodge's words found an echo in many men's minds; but the response was for a long while in men's minds only. In 1895, Sir Douglas Galton, having previously made a personal study of an institution analogous to the one desired—namely, the Reichsanstalt at Berlin—seized the opportunity offered to him as President of the Association at Ipswich to insist, with the authority not only of the head for the time being of a great scientific body, but also of one who himself knew the ways and wants at once of science and of practical life, that the thing which Lodge and others had hoped for was a thing which could be done, and ought to be done at once. And now to-day we can say it has been done. The National Physical Laboratory has been founded. The Address at Ipswich marked the beginning of an organised effort which has at last been crowned with success. A feeling of sadness cannot but come over us when we think that Sir Douglas Galton was not spared to see the formal completion of the scheme whose birth he did so much to help, and which, to his last days, he aided in more ways than one. It is the old story—the good which men do lives after them.

Still older than the Association is this nineteenth century, now swiftly drawing to its close. Though the century itself has yet some sixteen months to run, this is the last meeting of the British Association which will use the numbers eighteen hundred to mark its date.

The eyes of the young look ever forward; they take little heed of the short though ever-lengthening fragment of life which lies behind them; they are wholly bent on that which is to come. The eyes of the aged turn wistfully again and again to the past; as the old glide down the inevitable slope their present becomes a living over again the life which has gone before, and the future takes on the shape of a brief lengthening of the past. May I this evening venture to give rein to the impulses of advancing years? May I, at this last meeting of the Association in the eighteen hundreds, dare to dwell for a while upon the past, and to call to mind a few of the changes which have taken place in the world since those autumn days in which men were saying to each other that the last of the seventeen hundreds was drawing towards its end?

Dover in the year of our Lord seventeen hundred and ninety-nine was in many ways unlike the Dover of to-day. On moonless nights men groped their way in its narrow streets by the help of swinging lanterns

and smoky torches, for no lamps lit the ways. By day the light of the sun struggled into the houses through narrow panes of blurred glass. Though the town then, as now, was one of the chief portals to and from the countries beyond the seas, the means of travel were scanty and dear, available for the most part to the rich alone, and, for all, beset with discomfort and risk. Slow and uncertain was the carriage of goods, and the news of the world outside came to the town—though it from its position learnt more than most towns—tardily, fitfully, and often falsely. The people of Dover sat then much in dimness, if not in darkness, and lived in large measure on themselves. They who study the phenomena of living beings tell us that light is the great stimulus of life, and that the fulness of the life of a being or of any of its members may be measured by the variety, the swiftness, and the certainty of the means by which it is in touch with its surroundings. Judged from this standpoint life at Dover then, as indeed elsewhere, must have fallen far short of the life of to-day.

The same study of living beings, however, teaches us that while from one point of view the environment seems to mould the organism, from another point the organism seems to be master of its environment. Going behind the change of circumstances, we may raise the question, the old question, Was life in its essence worth more then than now? Has there been a real advance?

Let me at once relieve your minds by saying that I propose to leave this question in the main unanswered. It may be, or it may not be, that man's grasp of the beautiful and of the good, if not looser, is not firmer than it was a hundred years ago. It may be, or it may not be, that man is no nearer to absolute truth, to seeing things as they really are, than he was then. I will merely ask you to consider with me for a few minutes how far, and in what ways, man's laying hold of that aspect of or part of truth which we call natural knowledge, or sometimes science, differed in 1799 from what it is to-day, and whether that change must not be accounted a real advance, a real improvement in man.

I do not propose to weary you by what in my hands would be the rash effort of attempting a survey of all the scientific results of the nineteenth century. It will be enough if for a little while I dwell on some few of the salient features distinguishing the way in which we nowadays look upon, and during the coming week shall speak of, the works of Nature around us—though those works themselves, save for the slight shifting involved in a secular change, remain exactly the same—from the way in which they were looked upon and might have been spoken of at a gathering of philosophers at Dover in 1799. And I ask your leave to do so.

In the philosophy of the ancients, earth, fire, air, and water were called 'the elements.' It was thought, and rightly thought, that a knowledge of them and of their attributes was a necessary basis of a knowledge of the ways of Nature. Translated into modern language, a knowledge of

these 'elements' of old means a knowledge of the composition of the atmosphere, of water, and of all the other things which we call matter, as well as a knowledge of the general properties of gases, liquids, and solids, and of the nature and effects of combustion. Of all these things our knowledge to-day is large and exact, and, though ever enlarging, in some respects complete. When did that knowledge begin to become exact?

To-day the children in our schools know that the air which wraps round the globe is not a single thing, but is made up of two things, oxygen and nitrogen,¹ mingled together. They know, again, that water is not a single thing, but the product of two things, oxygen and hydrogen, joined together. They know that when the air makes the fire burn and gives the animal life, it is the oxygen in it which does the work. They know that all round them things are undergoing that union with oxygen which we call oxidation, and that oxidation is the ordinary source of heat and light. Let me ask you to picture to yourselves what confusion there would be to-morrow, not only in the discussions at the sectional meetings of our Association, but in the world at large, if it should happen that in the coming night some destroying touch should wither up certain tender structures in all our brains, and wipe out from our memories all traces of the ideas which cluster in our minds around the verbal tokens, oxygen and oxidation. How could any of us, not the so-called man of science alone, but even the man of business and the man of pleasure, go about his ways lacking those ideas? Yet those ideas were in 1799 lacking to all but a few.

Although in the third quarter of the seventeenth century the light of truth about oxidation and combustion had flashed out in the writings of John Mayow, it came as a flash only, and died away as soon as it had come. For the rest of that century, and for the greater part of the next, philosophers stumbled about in darkness, misled for the most of the time by the phantom conception which they called phlogiston. It was not until the end of the third quarter of the eighteenth century that the new light, which has burned steadily ever since, lit up the minds of the men of science. The light came at nearly the same time from England and from France. Rounding off the sharp corners of controversy, and joining, as we may fitly do to-day, the two countries as twin bearers of a common crown, we may say that we owe the truth to Cavendish, to Lavoisier, and Priestley. If it was Priestley who was the first to demonstrate the existence of what we now call oxygen, it is to Lavoisier we owe the true conception of the nature of oxidation and the clear exposition of the full meaning of Priestley's discovery, while the knowledge of the composition of water, the necessary complement of the knowledge of oxygen, came to us through Cavendish and, we may perhaps add, through Watt.

The date of Priestley's discovery of oxygen is 1774, Lavoisier's classic memoir 'on the nature of the principle which enters into combination

¹ Some may already know that there is at least a third thing, argon.

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with metals during calcination' appeared in 1775, and Cavendish's paper on the composition of water did not see the light until 1784.

During the last quarter of the eighteenth century this new idea of oxygen and oxidation was struggling into existence. How new was the idea is illustrated by the fact that Lavoisier himself at first spoke of that which he was afterwards, namely in 1778, led to call oxygen, the name by which it has since been known, as 'the principle which enters into combination.' What difficulties its acceptance met with is illustrated by the fact that Priestley himself refused to the end of his life to grasp the true bearings of the discovery which he had made. In the year 1799 the knowledge of oxygen, of the nature of water and of air, and indeed the true conception of chemical composition and chemical change, was hardly more than beginning to be, and the century had to pass wholly away before the next great chemical idea, which we know by the name of the Atomic Theory of John Dalton, was made known. We have only to read the scientific literature of the time to recognise that a truth which is now not only woven as a master-thread into all our scientific conceptions, but even enters largely into the everyday talk and thoughts of educated people, was a hundred years ago struggling into existence among the philosophers themselves. It was all but absolutely unknown to the large world outside those select few.

If there be one word of science which is writ large on the life of the present time, it is the word 'electricity'; it is, I take it, writ larger than any other word. The knowledge which it denotes has carried its practical results far and wide into our daily life, while the theoretical conceptions which it signifies pierce deep into the nature of things. We are to-day proud, and justly proud, both of the material triumphs and of the intellectual gains which it has brought us, and we are full of even larger hopes of it in the future.

At what time did this bright child of the nineteenth century have its birth?

He who listened to the small group of philosophers of Dover, who in 1799 might have discoursed of natural knowledge, would perhaps have heard much of electric machines, of electric sparks, of the electric fluid, and even of positive and negative electricity; for frictional electricity had long been known and even carefully studied. Probably one or more of the group, dwelling on the observations which Galvani, an Italian, had made known some twenty years before, developed views on the connection of electricity with the phenomena of living bodies. Possibly one of them was exciting the rest by telling how he had just heard that a professor at Pavia, one Volta, had discovered that electricity could be produced, not only by rubbing together particular bodies, but by the simple contact of two metals, and had thereby explained Galvani's remarkable results. For, indeed, as we shall hear from Professor Fleming, it was in that

very year, 1799, that electricity as we now know it took its birth. It was then that Volta brought to light the apparently simple truths out of which so much has sprung. The world, it is true, had to wait for yet some twenty years, before both the practical and the theoretic worth of Volta's discovery became truly pregnant, under the fertilising influence of another discovery. The loadstone and magnetic virtues had, like the electrifying power of rubbed amber, long been an old story. But, save for the compass, not much had come from it. And even Volta's discovery might have long remained relatively barren had it been left to itself. When, however, in 1819, Oersted made known his remarkable observations on the relations of electricity to magnetism, he made the contact needed for the flow of a new current of ideas. And it is perhaps not too much to say that those ideas, developing during the years of the rest of the century with an ever-accelerating swiftness, have wholly changed man's material relations to the circumstances of life, and at the same time carried him far in his knowledge of the nature of things.

Of all the various branches of science, none perhaps is to-day, none for these many years past has been, so well known to, even if not understood by, most people as that of geology. Its practical lessons have brought wealth to many ; its fairy tales have brought delight to more ; and round it hovers the charm of danger, for the conclusions to which it leads touch on the nature of man's beginning.

In 1799, the science of geology, as we now know it, was struggling into birth. There had been from of old cosmogonies, theories as to how the world had taken shape out of primæval chaos. In that fresh spirit which marked the zealous search after natural knowledge pursued in the middle and latter part of the seventeenth century, the brilliant Stenson, in Italy, and Hooke, in our own country, had laid hold of some of the problems presented by fossil remains, and Woodward, with others, had laboured in the same field. In the eighteenth century, especially in its latter half, men's minds were busy about the physical agencies determining or modifying the features of the earth's crust ; water and fire, subsidence from a primæval ocean and transformation by outbursts of the central heat, Neptune and Pluto, were being appealed to, by Werner on the one hand, and by Desmarest on the other, in explanation of the earth's phenomena. The way was being prepared, theories and views were abundant, and many sound observations had been made ; and yet the science of geology, properly so called, the exact and proved knowledge of the successive phases of the world's life, may be said to date from the closing years of the eighteenth century.

In 1783, James Hutton put forward in a brief memoir his 'Theory of the Earth,' which in 1795, two years before his death, he expanded into a book ; but his ideas failed to lay hold of men's minds until the century had

passed away, when, in 1802, they found an able expositor in John Playfair. The very same year that Hutton published his theory, Cuvier came to Paris and almost forthwith began, with Brongniart, his immortal researches into the fossils of Paris and its neighbourhood. And four years later, in the year 1799 itself, William Smith's tabular list of strata and fossils saw the light. It is, I believe, not too much to say that out of these geology, as we now know it, sprang. It was thus in the closing years of the eighteenth century that was begun the work which the nineteenth century has carried forward to such great results. But at that time only the select few had grasped the truth, and even they only the beginning of it. Outside a narrow circle the thoughts, even of the educated, about the history of the globe were bounded by the story of the Deluge—though the story was often told in a strange fashion—or were guided by fantastic views of the plastic forces of a sportive Nature.

In another branch of science, in that which deals with the problems presented by living beings, the thoughts of men in 1799 were also very different from the thoughts of men to-day. It is a very old quest, the quest after the knowledge of the nature of living beings, one of the earliest on which man set out; for it promised to lead him to a knowledge of himself, a promise which perhaps is still before us, but the fulfilment of which is as yet far off. As time has gone on, the pursuit of natural knowledge has seemed to lead man away from himself into the furthestmost parts of the universe, and into secret workings of Nature in which he appears to be of little or no account; and his knowledge of the nature of living things, and so of his own nature, has advanced slowly, waiting till the progress of other branches of natural knowledge can bring it aid. Yet in the past hundred years, the biologic sciences, as we now call them, have marched rapidly onward.

We may look upon a living body as a machine doing work in accordance with certain laws, and may seek to trace out the working of the inner wheels, how these raise up the lifeless dust into living matter, and let the living matter fall away again into dust, giving out movement and heat. Or we may look upon the individual life as a link in a long chain, joining something which went before to something about to come, a chain whose beginning lies hid in the farthest past, and may seek to know the ties which bind one life to another. As we call up to view the long series of living forms, living now or flitting like shadows on the screen of the past, we may strive to lay hold of the influences which fashion the garment of life. Whether the problems of life are looked upon from the one point of view or the other, we to-day, not biologists only, but all of us, have gained a knowledge hidden even from the philosophers a hundred years ago.

Of the problems presented by the living body viewed as a machine, some may be spoken of as mechanical, others as physical, and yet others

as chemical, while some are, apparently at least, none of these. In the seventeenth century William Harvey, laying hold of the central mechanism of the blood stream, opened up a path of inquiry which his own age and the century which followed trod with marked success. The knowledge of the mechanics of the animal and of the plant advanced apace ; but the physical and chemical problems had yet to wait. The eighteenth century, it is true, had its physics and its chemistry ; but, in relation at least to the problems of the living being, a chemistry which knew not oxygen and a physics which knew not the electricity of chemical action were of little avail. The philosopher of 1799, when he discussed the functions of the animal or of the plant involving chemical changes, was fain for the most part, as were his predecessors in the century before, to have recourse to such vague terms as 'fermentation' and the like ; to-day our treatises on physiology are largely made up of precise and exact expositions of the play of physical agencies and chemical bodies in the living organism. He made use of the words 'vital force' or 'vital principle' not as an occasional, but as a common, explanation of the phenomena of the living body. During the present century, especially during its latter half, the idea embodied in those words has been driven away from one seat after another ; if we use it now when we are dealing with the chemical and physical events of life we use it with reluctance, as a *deus ex machina* to be appealed to only when everything else has failed.

Some of the problems—and those, perhaps, the chief problems—of the living body have to be solved neither by physical nor by chemical methods, but by methods of their own. Such are the problems of the nervous system. In respect to these the men of 1799 were on the threshold of a pregnant discovery. During the latter part of the present century, and especially during its last quarter, the analysis of the mysterious processes in the nervous system, and especially in the brain, which issue as feeling, thought, and the power to move, has been pushed forward with a success conspicuous in its practical, and full of promise in its theoretical, gains. That analysis may be briefly described as a following up of threads. We now know that what takes place along a tiny thread which we call a nerve-fibre differs from that which takes place along its fellow-threads, that differing nervous impulses travel along different nerve-fibres, and that nervous and psychical events are the outcome of the clashing of nervous impulses as they sweep along the closely-woven web of living threads of which the brain is made. We have learnt by experiment and by observation that the pattern of the web determines the play of the impulses, and we can already explain many of the obscure problems not only of nervous disease, but of nervous life, by an analysis which is a tracking out the devious and linked paths of nervous threads. The very beginning of this analysis was unknown in 1799. Men knew that nerves were the agents of feeling and of the movements of muscles ; they had learnt much about what this part or that part of the brain could do ; but they did not know that

one nerve-fibre differed from another in the very essence of its work. It was just about the end of the past century, or the beginning of the present one, that an English surgeon began to ponder over a conception which, however, he did not make known until some years later, and which did not gain complete demonstration and full acceptance until still more years had passed away. It was in 1811, in a tiny pamphlet published privately, that Charles Bell put forward his 'New Idea' that the nervous system was constructed on the principle that 'the nerves are not single nerves possessing various powers, but bundles of different nerves, whose filaments are united for the convenience of distribution, but which are distinct in office as they are in origin from the brain.'

Our present knowledge of the nervous system is to a large extent only an exemplification and expansion of Charles Bell's 'New Idea,' and has its origin in that.

If we pass from the problems of the living organism viewed as a machine to those presented by the varied features of the different creatures who have lived or who still live on the earth, we at once call to mind that the middle years of the present century mark an epoch in biologic thought such as never came before, for it was then that Charles Darwin gave to the world the 'Origin of Species.'

That work, however, with all the far-reaching effects which it has had, could have had little or no effect, or, rather, could not have come into existence, had not the earlier half of the century been in travail preparing for its coming. For the germinal idea of Darwin appeals, as to witnesses, to the results of two lines of biologic investigation which were almost unknown to the men of the eighteenth century.

To one of these lines I have already referred. Darwin, as we know, appealed to the geological record; and we also know how that record, imperfect as it was then, and imperfect as it must always remain, has since his time yielded the most striking proofs of at least one part of his general conception. In 1799 there was, as we have seen, no geological record at all.

Of the other line I must say a few words.

To-day the merest beginner in biologic study, or even that exemplar of acquaintance without knowledge, the general reader, is aware that every living being, even man himself, begins its independent existence as a tiny ball, of which we can, even acknowledging to the full the limits of the optical analysis at our command, assert with confidence that in structure, using that word in its ordinary sense, it is in all cases absolutely simple. It is equally well known that the features of form which supply the characters of a grown-up living being, all the many and varied features of even the most complex organism, are reached as the goal of a road, at times a long road, of successive changes; that the life of every being, from the ovum to its full estate, is a series of shifting scenes, which come and go, sometimes changing abruptly, sometimes melting the one into the

other, like dissolving views, all so ordained that often the final shape with which the creature seems to begin, or is said to begin, its life in the world is the outcome of many shapes, clothed with which it in turn has lived many lives before its seeming birth.

All or nearly all the exact knowledge of the laboured way in which each living creature puts on its proper shape and structure is the heritage of the present century. Although the way in which the chick is moulded in the egg was not wholly unknown even to the ancients, and in later years had been told, first in the sixteenth century by Fabricius, then in the seventeenth century in a more clear and striking manner by the great Italian naturalist Malpighi, the teaching thus offered had been neglected or misinterpreted. At the close of the eighteenth century the dominant view was that in the making of a creature out of the egg there was no putting on of wholly new parts, no epigenesis. It was taught that the entire creature lay hidden in the egg, hidden by reason of the very transparency of its substance, lay ready-made but folded up, as it were, and that the process of development within the egg or within the womb was a mere unfolding, a simple evolution. Nor did men shrink from accepting the logical outcome of such a view—namely, that within the unborn creature itself lay in like manner, hidden and folded up, its offspring also, and within that again its offspring in turn, after the fashion of a cluster of ivory balls carved by Chinese hands, one within the other. This was no fantastic view put forward by an imaginative dreamer; it was seriously held by sober men, even by men like the illustrious Haller, in spite of their recognising that as the chick grew in the egg some changes of form took place. Though so early as the middle of the eighteenth century Friedrich Caspar Wolff and, later on, others had strenuously opposed such a view, it held its own not only to the close of the century, but far on into the next. It was not until a quarter of the present century had been added to the past that Von Baer made known the results of researches which once and for all swept away the old view. He and others working after him made it clear that each individual puts on its final form and structure not by an unfolding of pre-existing hidden features, but by the formation of new parts through the continued differentiation of a primitively simple material. It was also made clear that the successive changes which the embryo undergoes in its progress from the ovum to maturity are the expression of morphologic laws, that the progress is one from the general to the special, and that the shifting scenes of embryonic life are hints and tokens of lives lived by ancestors in times long past.

If we wish to measure how far off in biologic thought the end of the last century stands, not only from the end but even from the middle of this one, we may imagine Darwin striving to write the 'Origin of Species' in 1799. We may fancy him being told by philosophers explaining how one group of living beings differed from another group because all its members

and all their ancestors came into existence at one stroke when the first-born progenitor of the race, within which all the rest were folded up, stood forth as the result of a creative act. We may fancy him listening to a debate between the philosopher who maintained that all the fossils strewn in the earth were the remains of animals or plants churned up in the turmoil of a violent universal flood, and dropped in their places as the waters went away, and him who argued that such were not really the 'spoils of living creatures,' but the products of some playful 'plastic power which out of the superabundance of its energy fashioned here and there the lifeless earth into forms which imitated, but only imitated, those of living things. Could he amid such surroundings by any flight of genius have beat his way to the conception for which his name will ever be known?

Here I may well turn away from the past. It is not my purpose, nor, as I have said, am I fitted, nor is this perhaps the place, to tell even in outline the tale of the work of science in the nineteenth century. I am content to have pointed out that the two great sciences of chemistry and geology took their birth, or at least began to stand alone, at the close of the last century, and have grown to be what we know them now within about a hundred years, and that the study of living beings has within the same time been so transformed as to be to-day something wholly different from what it was in 1799. And, indeed, to say more would be to repeat almost the same story about other things. If our present knowledge of electricity is essentially the child of the nineteenth century, so also is our present knowledge of many other branches of physics. And those most ancient forms of exact knowledge, the knowledge of numbers and of the heavens, whose beginning is lost in the remote past, have, with all other kinds of natural knowledge, moved onward during the whole of the hundred years with a speed which is ever increasing. I have said, I trust, enough to justify the statement that in respect to natural knowledge a great gulf lies between 1799 and 1899. That gulf, moreover, is a two-fold one: not only has natural knowledge been increased, but men have run to and fro spreading it as they go. Not only have the few driven far back round the full circle of natural knowledge the dark clouds of the unknown which wrap us all about, but also the many walk in the zone of light thus increasingly gained. If it be true that the few to-day are, in respect to natural knowledge, far removed from the few of those days, it is also true that nearly all which the few alone knew then, and much which they did not know, has now become the common knowledge of the many.

What, however, I may venture to insist upon here is that the difference in respect to natural knowledge, whatever be the case with other differences between then and now, is undoubtedly a difference which means progress. The span between the science of that time and the science of to-day is beyond all question a great stride onwards.

We may say this, but we must say it without boasting. For the very story of the past which tells of the triumphs of science bids the man of science put away from him all thoughts of vainglory. And that by many tokens.

Whoever, working at any scientific problem, has occasion to study the inquiries into the same problem made by some fellow-worker in the years long gone by, comes away from that study humbled by one or other of two different thoughts. On the one hand he may find, when he has translated the language of the past into the phraseology of to-day, how near was his forerunner of old to the conception which he thought, with pride, was all his own, not only so true but so new. On the other hand, if the ideas of the investigator of old, viewed in the light of modern knowledge, are found to be so wide of the mark as to seem absurd, the smile which begins to play upon the lips of the modern is checked by the thought, Will the ideas which I am now putting forth, and which I think explain so clearly, so fully, the problem in hand, seem to some worker in the far future as wrong and as fantastic as do these of my forerunner to me? In either case his personal pride is checked. Further, there is written clearly on each page of the history of science, in characters which cannot be overlooked, the lesson that no scientific truth is born anew, coming by itself and of itself. Each new truth is always the offspring of something which has gone before, becoming in turn the parent of something coming after. In this aspect the man of science is unlike, or seems to be unlike, the poet and the artist. The poet is born, not made; he rises up, no man knowing his beginnings; when he goes away, though men after him may sing his songs for centuries, he himself goes away wholly, having taken with him his mantle, for this he can give to none other. The man of science is not thus creative; he is created. His work, however great it be, is not wholly his own; it is in part the outcome of the work of men who have gone before. Again and again a conception which has made a name great has come not so much by the man's own effort as out of the fulness of time. Again and again we may read in the words of some man of old the outlines of an idea which in later days has shone forth as a great acknowledged truth. From the mouth of the man of old the idea dropped barren, fruitless; the world was not ready for it, and heeded it not; the concomitant and abutting truths which could give it power to work were wanting. Coming back again in later days, the same idea found the world awaiting it; things were in travail preparing for it; and someone, seizing the right moment to put it forth again, leapt into fame. It is not so much the men of science who make science, as some spirit which, born of the truths already won, drives the man of science onward and uses him to win new truths in turn.

It is because each man of science is not his own master, but one of many obedient servants of an impulse which was at work long before him, and will work long after him, that in science there is no falling back. In

respect to other things there may be times of darkness and times of light, there may be risings, decadences, and revivals. In science there is only progress. The path may not be always a straight line, there may be swerving to this side and to that, ideas may seem to return again and again to the same point of the intellectual compass ; but it will always be found that they have reached a higher level—they have moved, not in a circle, but in a spiral. Moreover science is not fashioned as is a house, by putting brick to brick, that which is once put remaining as it was put to the end. The growth of science is that of a living being. As in the embryo phase follows phase, and each member of the body puts on in succession different appearances, though all the while the same member, so a scientific conception of one age seems to differ from that of a following age, though it is the same one in the process of being made ; and as the dim outlines of the early embryo become, as the being grows more distinct and sharp, like a picture on a screen brought more and more into focus, so the dim gropings and searchings of the men of science of old are by repeated approximations wrought into the clear and exact conclusions of later times.

The story of natural knowledge, of science, in the nineteenth century, as, indeed, in preceding centuries, is, I repeat, a story of continued progress. There is in it not so much as a hint of falling back, not even of standing still. What is gained by scientific inquiry is gained for ever ; it may be added to, it may seem to be covered up, but it can never be taken away. Confident that the progress will go on, we cannot help peering into the years to come and straining our eyes to foresee what science will become and what it will do as they roll on. While we do so, the thought must come to us, Will all the increasing knowledge of Nature avail only to change the ways of man—will it have no effect on man himself ?

The material good which mankind has gained and is gaining through the advance of science is so imposing as to be obvious to everyone, and the praises of this aspect of science are to be found in the mouths of all. Beyond all doubt science has greatly lessened and has markedly narrowed hardship and suffering ; beyond all doubt science has largely increased and has widely diffused ease and comfort. The appliances of science have, as it were, covered with a soft cushion the rough places of life, and that not for the rich only, but also for the poor. So abundant and so prominent are the material benefits of science that in the eyes of many these seem to be the only benefits which she brings. She is often spoken of as if she were useful and nothing more, as if her work were only to administer to the material wants of man.

Is this so ?

We may begin to doubt it when we reflect that the triumphs of science which bring these material advantages are in their very nature intellectual triumphs. The increasing benefits brought by science are the results

of man's increasing mastery over Nature, and that mastery is increasingly a mastery of mind ; it is an increasing power to use the forces of what we call inanimate nature in place of the force of his own or other creatures' bodies ; it is an increasing use of mind in place of muscle.

Is it to be thought that that which has brought the mind so greatly into play has had no effect on the mind itself ? Is that part of the mind which works out scientific truths a mere slavish machine producing results it knows not how, having no part in the good which in its working it brings forth ?

What are the qualities, the features of that scientific mind which has wrought, and is working, such great changes in man's relation to Nature ? In seeking an answer to this question we have not to inquire into the attributes of genius. Though much of the progress of science seems to take on the form of a series of great steps, each made by some great man, the distinction in science between the great discoverer and the humble worker is one of degree only, not of kind. As I was urging just now, the greatness of many great names in science is often, in large part, the greatness of occasion, not of absolute power. The qualities which guide one man to a small truth silently taking its place among its fellows, as these go to make up progress, are at bottom the same as those by which another man is led to something of which the whole world rings.

The features of the fruitful scientific mind are in the main three.

In the first place, above all other things, his nature must be one which vibrates in unison with that of which he is in search ; the seeker after truth must himself be truthful, truthful with the truthfulness of Nature. For the truthfulness of Nature is not wholly the same as that which man sometimes calls truthfulness. It is far more imperious, far more exacting. Man, unscientific man, is often content with 'the nearly' and 'the almost.' Nature never is. It is not her way to call the same two things which differ, though the difference may be measured by less than the thousandth of a milligramme or of a millimetre, or by any other like standard of minuteness. And the man who, carrying the ways of the world into the domain of science, thinks that he may treat Nature's differences in any other way than she treats them herself, will find that she resents his conduct ; if he in carelessness or in disdain overlooks the minute difference which she holds out to him as a signal to guide him in his search, the projecting tip, as it were, of some buried treasure, he is bound to go astray, and the more strenuously he struggles on, the farther will he find himself from his true goal.

In the second place, he must be alert of mind. Nature is ever making signs to us, she is ever whispering to us the beginnings of her secrets ; the scientific man must be ever on the watch, ready at once to lay hold of Nature's hint however small, to listen to her whisper however low.

In the third place, scientific inquiry, though it be pre-eminently an intellectual effort, has need of the moral quality of courage—not so much the courage which helps a man to face a sudden difficulty as the courage

of steadfast endurance. Almost every inquiry, certainly every prolonged inquiry, sooner or later goes wrong. The path, at first so straight and clear, grows crooked and gets blocked ; the hope and enthusiasm, or even the jaunty ease, with which the inquirer set out leave him and he falls into a slough of despond. That is the critical moment calling for courage. Struggling through the slough he will find on the other side the wicket-gate opening up the real path ; losing heart he will turn back and add one more stone to the great cairn of the unaccomplished.

But, I hear someone say, these qualities are not the peculiar attributes of the man of science, they may be recognised as belonging to almost everyone who has commanded or deserved success, whatever may have been his walk of life. That is so. That is exactly what I would desire to insist, that the men of science have no peculiar virtues, no special powers. They are ordinary men, their characters are common, even commonplace. Science, as Huxley said, is organised common sense, and men of science are common men, drilled in the ways of common sense.

For their life has this feature. Though in themselves they are no stronger, no better than other men, they possess a strength which, as I just now urged, is not their own but is that of the science whose servants they are. Even in his apprenticeship, the scientific inquirer, while learning what has been done before his time, if he learns it aright, so learns it that what is known may serve him not only as a vantage ground whence to push off into the unknown, but also as a compass to guide him in his course. And when fitted for his work he enters on inquiry itself, what a zealous anxious guide, what a strict and, because strict, helpful school-mistress does Nature make herself to him ! Under her care every inquiry, whether it bring the inquirer to a happy issue or seem to end in nought, trains him for the next effort. She so orders her ways that each act of obedience to her makes the next act easier for him, and step by step she leads him on towards that perfect obedience which is complete mastery.

Indeed, when we reflect on the potency of the discipline of scientific inquiry we cease to wonder at the progress of scientific knowledge. The results actually gained seem to fall so far short of what under such guidance might have been expected to have been gathered in that we are fain to conclude that science has called to follow her, for the most part, the poor in intellect and the wayward in spirit. Had she called to her service the many acute minds who have wasted their strength struggling in vain to solve hopeless problems, or who have turned their energies to things other than the increase of knowledge ; had she called to her service the many just men who have walked straight without the need of a rod to guide them, how much greater than it has been would have been the progress of science, and how many false teachings would the world have been spared ! To men of science themselves, when they consider their favoured lot, the achievements of the past should serve not as a boast, but as a reproach.

If there be any truth in what I have been urging, that the pursuit of scientific inquiry is itself a training of special potency, giving strength to the feeble and keeping in the path those who are inclined to stray, it is obvious that the material gains of science, great as they may be, do not make up all the good which science brings or may bring to man. We especially, perhaps, in these later days, through the rapid development of the physical sciences, are too apt to dwell on the material gains alone. As a child in its infancy looks upon its mother only as a giver of good things, and does not learn till in after days how she was also showing her love by carefully training it in the way it should go, so we, too, have thought too much of the gifts of science, overlooking her power to guide.

Man does not live by bread alone, and science brings him more than bread. It is a great thing to make two blades of grass grow where before one alone grew ; but it is no less great a thing to help a man to come to a just conclusion on the questions with which he has to deal. We may claim for science that while she is doing the one she may be so used as to do the other also. The dictum just quoted, that science is organised common sense, may be read as meaning that the common problems of life which common people have to solve are to be solved by the same methods by which the man of science solves his special problems. It follows that the training which does so much for him may be looked to as promising to do much for them. Such aid can come from science on two conditions only. In the first place, this her influence must be acknowledged ; she must be duly recognised as a teacher no less than as a hewer of wood and a drawer of water. And the pursuit of science must be followed not by the professional few only, but, at least in such measure as will ensure the influence of example, by the many. But this latter point I need not urge before this great Association, whose chief object during more than half a century has been to bring within the fold of science all who would answer to the call. In the second place, it must be understood that the training to be looked for from science is the outcome not of the accumulation of scientific knowledge, but of the practice of scientific inquiry. Man may have at his fingers' ends all the accomplished results and all the current opinions of any one or of all the branches of science, and yet remain wholly unscientific in mind ; but no one can have carried out even the humblest research without the spirit of science in some measure resting upon him. And that spirit may in part be caught even without entering upon an actual investigation in search of a new truth. The learner may be led to old truths, even the oldest, in more ways than one. He may be brought abruptly to a truth in its finished form, coming straight to it like a thief climbing over the wall ; and the hurry and press of modern life tempt many to adopt this quicker way. Or he may be more slowly guided along the path by which the truth was reached by him who first laid hold of it. It is by this latter way of learning the truth, and by this

alone, that the learner may hope to catch something at least of the spirit of the scientific inquirer.

This is not the place, nor have I the wish, to plunge into the turmoil of controversy ; but, if there be any truth in what I have been urging, then they are wrong who think that in the schooling of the young science can be used with profit only to train those for whom science will be the means of earning their bread. It may be that from the point of view of the pedagogic art the experience of generations has fashioned out of the older studies of literature an instrument of discipline of unusual power, and that the teaching of science is as yet but a rough tool in unpractised hands. That, however, is not an adequate reason why scope should not be given for science to show the value which we claim for it as an intellectual training fitted for all sorts and conditions of men. Nor need the studies of humanity and literature fear her presence in the schools, for if her friends maintain that that teaching is one-sided, and therefore misleading, which deals with the doings of man only, and is silent about the works of Nature, in the sight of which he and his doings shrink almost to nothing, she herself would be the first to admit that that teaching is equally wrong which deals only with the works of Nature and says nothing about the doings of man, who is, to us at least, Nature's centre.

There is yet another general aspect of science on which I would crave leave to say a word. In that broad field of human life which we call politics, in the struggle not of man with man, but of race with race, science works for good. If we look only on the surface it may at first sight seem otherwise. In no branch of science has there during these later years been greater activity and more rapid progress than in that which furnishes the means by which man brings death, suffering, and disaster on his fellow-men. If the healer can look with pride on the increased power which science has given him to alleviate human suffering and ward off the miseries of disease, the destroyer can look with still greater pride on the power which science has given him to sweep away lives and to work desolation and ruin ; while the one has slowly been learning to save units, the other has quickly learnt to slay thousands. But, happily, the very greatness of the modern power of destruction is already becoming a bar to its use, and bids fair—may we hope before long ?—wholly to put an end to it ; in the words of Tacitus, though in another sense, the very preparations for war, through the character which science gives them, make for peace.

Moreover, not in one branch of science only, but in all, there is a deep undercurrent of influence sapping the very foundations of all war. As I have already urged, no feature of scientific inquiry is more marked than the dependence of each step forward on other steps which have been made before. The man of science cannot sit by himself in his own cave weaving

out results by his own efforts, unaided by others, heedless of what others have done and are doing. He is but a bit of a great system, a joint in a great machine, and he can only work aright when he is in due touch with his fellow-workers. If his labour is to be what it ought to be, and is to have the weight which it ought to have, he must know what is being done, not by himself, but by others, and by others not of his own land and speaking his tongue only, but also of other lands and of other speech. Hence it comes about that to the man of science the barriers of manners and of speech which pen men into nations become more and more unreal and indistinct. He recognises his fellow-worker, wherever he may live and whatever tongue he may speak, as one who is pushing forward shoulder to shoulder with him towards a common goal, as one whom he is helping and who is helping him. The touch of science makes the whole world kin.

The history of the past gives us many examples of this brotherhood of science. In the revival of learning throughout the sixteenth and seventeenth centuries, and some way on into the eighteenth century, the common use of the Latin tongue made intercourse easy. In some respects in those earlier days science was more cosmopolitan than it afterwards became. In spite of the difficulties and hardships of travel, the men of science of different lands again and again met each other face to face, heard with their ears, and saw with their eyes what their brethren had to say or to show. The Englishman took the long journey to Italy to study there; the Italian, the Frenchman, and the German wandered from one seat of learning to another; and many a man held a chair in a country not his own. There was help, too, as well as intercourse. The Royal Society of London took upon itself the task of publishing nearly all the works of the great Italian Malpighi, and the brilliant Lavoisier, two years before his own countrymen in their blind fury slew him, received from the same body the highest token which it could give of its esteem.

In these closing years of the nineteenth century this great need of mutual knowledge and of common action felt by men of science of different lands is being manifested in a special way. Though nowadays what is done anywhere is soon known everywhere, the news of a discovery being often flashed over the globe by telegraph, there is an increasing activity in the direction of organisation to promote international meetings and international co-operation. In almost every science inquirers from many lands now gather together at stated intervals in international congresses to discuss matters which they have in common at heart, and go away each one feeling strengthened by having met his brother. The desire that in the struggle to lay bare the secrets of Nature the least waste of human energy should be incurred is leading more and more to the concerted action of nations combining to attack problems the solution of which is difficult and costly. The determination of standards of measurement, magnetic surveys, the solution of great geodetic problems, the mapping of the

heavens and of the earth—all these are being carried on by international organisations.

In this and in other countries men's minds have this long while past been greatly moved by the desire to make fresh efforts to pierce the dark secrets of the forbidding Antarctic regions. Belgium has just made a brave single-handed attempt; a private enterprise sailing from these shores is struggling there now, lost for the present to our view; and this year we in England and our brethren in Germany are, thanks to the promised aid of the respective Governments, and no less to private liberality, in which this Association takes its share, able to begin the preparation of carefully organised expeditions. That international amity of which I am speaking is illustrated by the fact that in this country and in that there is not only a great desire, but a firm purpose, to secure the fullest co-operation between the expeditions which will leave the two shores. If in this momentous attempt any rivalry be shown between the two nations, it will be for each a rivalry, not in forestalling, but in assisting the other. May I add that if the story of the past may seem to give our nation some claim to the seas as more peculiarly our own, that claim bespeaks a duty likewise peculiarly our own to leave no effort untried by which we may plumb the seas' yet unknown depths and trace their yet unknown shores? That claim, if it means anything, means that when nations are joining hands in the dangerous work of exploring the unknown South, the larger burden of the task should fall to Britain's share; it means that we in this country should see to it, and see to it at once, that the concerted Antarctic expedition which in some two years or so will leave the shores of Germany, of England, and, perhaps, of other lands, should, so far as we are concerned, be so equipped and so sustained that the risk of failure and disaster may be made as small, and the hope of being able not merely to snatch a hurried glimpse of lands not yet seen, but to gather in with full hands a rich harvest of the facts which men not of one science only, but of many, long to know, as great as possible.

Another international scientific effort demands a word of notice. The need which every inquirer in science feels to know, and to know quickly, what his fellow-worker, wherever on the globe he may be carrying on his work or making known his results, has done or is doing, led some four years back to a proposal for carrying out by international co-operation a complete current index, issued promptly, of the scientific literature of the world. Though much labour in many lands has been spent upon the undertaking, the project is not yet an accomplished fact. Nor can this, perhaps, be wondered at, when the difficulties of the task are weighed. Difficulties of language, difficulties of driving in one team all the several sciences which, like young horses, wish each to have its head free with leave to go its own way, difficulties mechanical and financial of press and post, difficulties raised by existing interests—these and yet other difficulties are obstacles not easy to be overcome. The most striking

and the most encouraging features of the deliberations which have now been going on for three years have been the repeated expressions, coming not from this or that quarter only, but from almost all quarters, of an earnest desire that the effort should succeed, of a sincere belief in the good of international co-operation, and of a willingness to sink as far as possible individual interests for the sake of the common cause. In the face of such a spirit we may surely hope that the many difficulties will ultimately pass out of sight.

Perhaps, however, not the least notable fact of international co-operation in science is the proposal which has been made within the last two years that the leading academies of the world should, by representatives, meet at intervals to discuss questions in which the learned of all lands are interested. A month hence a preliminary meeting of this kind will be held at Wiesbaden; and it is at least probable that the closing year of that nineteenth century in which science has played so great a part may at Paris during the great World's Fair—which every friend, not of science only, but of humanity, trusts may not be put aside or even injured through any untoward event, and which promises to be an occasion not of pleasurable sight-seeing only, but also, by its many international congresses, of international communing in the search for truth—witness the first select Witenagemote of the science of the world.

I make no apology for having thus touched on international co-operation. I should have been wanting, had I not done so, to the memorable occasion of this meeting. A hundred years ago two great nations were grappling with each other in a fierce struggle, which had lasted, with pauses, for many years, and was to last for many years to come; war was on every lip and in almost every heart. To-day this meeting has, by a common wish, been so arranged that those two nations should, in the persons of their men of science draw as near together as they can, with nothing but the narrow streak of the Channel between them, in order that they may take counsel together on matters in which they have one interest and a common hope. May we not look upon this brotherly meeting as one of many signs that science, though she works in a silent manner and in ways unseen by many, is steadily making for peace?

Looking back, then, in this last year of the eighteen hundreds, on the century which is drawing to its close, while we may see in the history of scientific inquiry much which, telling the man of science of his shortcomings and his weakness, bids him be humble, we also see much, perhaps more, which gives him hope. Hope is indeed one of the watchwords of science. In the latter-day writings of some who know not science, much may be read which shows that the writer is losing or has lost hope in the future of mankind. There are not a few of these; their repeated utterances make a sign of the times. Seeing in matters lying outside science few marks of progress and many tokens of decline or of decay, recognising

in science its material benefits only, such men have thoughts of despair when they look forward to the times to come. But if there be any truth in what I have attempted to urge to-night, if the intellectual, if the moral influences of science are no less marked than her material benefits, if, moreover, that which she has done is but the earnest of that which she shall do, such men may pluck up courage and gather strength by laying hold of her garment. We men of science at least need not share their views or their fears. Our feet are set, not on the shifting sands of the opinions and of the fancies of the day, but on a solid foundation of verified truth, which by the labours of each succeeding age is made broader and more firm. To us the past is a thing to look back upon, not with regret, not as something which has been lost never to be regained, but with content, as something whose influence is with us still, helping us on our further way. With us, indeed, the past points not to itself, but to the future ; the golden age is in front of us, not behind us ; that which we do know is a lamp whose brightest beams are shed into the unknown before us, showing us how much there is in front and lighting up the way to reach it. We are confident in the advance because, as each one of us feels that any step forward which he may make is not ordered by himself alone and is not the result of his own sole efforts in the present, but is, and that in large measure, the outcome of the labours of others in the past, so each one of us has the sure and certain hope that as the past has helped him, so his efforts, be they great or be they small, will be a help to those to come.

British Association for the Advancement of Science.

BRADFORD, 1900.

ADDRESS

BY

PROFESSOR SIR WILLIAM TURNER, M.B., D.C.L.,
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PRESIDENT.

TWENTY-SEVEN years ago the British Association met in Bradford, not at that time raised to the dignity of a City. The meeting was very successful, and was attended by about 2,000 persons—a forecast, let us hope, of what we may expect at the present assembly. A distinguished chemist, Professor A. W. Williamson, presided. On this occasion the Association has selected for the presidential chair one whose attention has been given to the study of an important department of biological science. His claim to occupy, however unworthily, the distinguished position in which he has been placed, rests, doubtless, on the fact that, in the midst of the engrossing duties devolving on a teacher in a great University and School of Medicine, he has endeavoured to contribute to the sum of knowledge of the science which he professes. It is a matter of satisfaction to feel that the success of a meeting of this kind does not rest upon the shoulders of the occupant of the presidential chair, but is due to the eminence and active co-operation of the men of science who either preside over or engage in the work of the nine or ten sections into which the Association is divided, and to the energy and ability for organisation displayed by the local Secretaries and Committees. The programme prepared by the general and local officers of the Association shows that no efforts have been spared to provide an ample bill of fare, both in its scientific and social aspects. Members and Associates will, I feel sure, take away from the Bradford Meeting as pleasant memories as did our colleagues of the corresponding Association Française, when, in friendly collaboration at Dover last year, they testified to the common citizenship of the Universal Republic of Science. As befits a leading centre of industry in the great county of York, the applications of science to the industrial arts and to agriculture will form subjects of discussion in the papers to be read at the meeting.

Since the Association was at Dover a year ago, two of its former Presidents have joined the majority. The Duke of Argyll presided at the meeting in Glasgow so far back as 1855. Throughout his long and energetic life, he proved himself to be an eloquent and earnest speaker, one who gave to the consideration of public affairs a mind of singular independence, and a thinker and writer in a wide range of human knowledge. Sir J. Wm. Dawson was President at the meeting in Birmingham in 1886. Born in Nova Scotia in 1820, he devoted himself to the study of the Geology of Canada, and became the leading authority on the subject. He took also an active and influential part in promoting the spread of scientific education in the Dominion, and for a number of years he was Principal and Vice-Chancellor of the M'Gill University, Montreal.

Scientific Method.

Edward Gibbon has told us that diligence and accuracy are the only merits which an historical writer can ascribe to himself. Without doubt they are fundamental qualities necessary for historical research, but in order to bear fruit they require to be exercised by one whose mental qualities are such as to enable him to analyse the data brought together by his diligence, to discriminate between the false and the true, to possess an insight into the complex motives that determine human action, to be able to recognise those facts and incidents which had exercised either a primary or only a secondary influence on the affairs of nations, or on the thoughts and doings of the person whose character he is depicting.

In scientific research, also, diligence and accuracy are fundamental qualities. By their application, new facts are discovered and tabulated, their order of succession is ascertained, and a wider and more intimate knowledge of the processes of nature is acquired. But to decide on their true significance a well-balanced mind and the exercise of prolonged thought and reflection are needed. William Harvey, the father of exact research in physiology, in his memorable work 'De Motu Cordis et Sanguinis,' published more than two centuries ago, tells us of the great and daily diligence which he exercised in the course of his investigations, and the numerous observations and experiments which he collated. At the same time he refers repeatedly to his cogitations and reflections on the meaning of what he had observed, without which the complicated movements of the heart could not have been analysed, their significance determined, and the circulation of the blood in a continuous stream definitely established. Early in the present century, Carl Ernst von Baer, the father of embryological research, showed the importance which he attached to the combination of observation with meditation by placing side by side on the title-page of his famous treatise 'Ueber Entwickelungsgeschichte der Thiere' (1828) the words *Beobachtung und Reflexion*.

Though I have drawn from biological science my illustrations of the need of this combination, it must not be inferred that it applies exclu-

sively to one branch of scientific inquiry ; the conjunction influences and determines progress in all the sciences, and when associated with a sufficient touch of imagination, when the power of seeing is conjoined with the faculty of foreseeing, of projecting the mind into the future, we may expect something more than the discovery of isolated facts ; their co-ordination and the enunciation of new principles and laws will necessarily follow.

Scientific method consists, therefore, in close observation, frequently repeated so as to eliminate the possibility of erroneous seeing ; in experiments checked and controlled in every direction in which fallacies might arise ; in continuous reflection on the appearances and phenomena observed, and in logically reasoning out their meaning and the conclusions to be drawn from them. Were the method followed out in its integrity by all who are engaged in scientific investigations, the time and labour expended in correcting errors committed by ourselves or by other observers and experimentalists would be saved, and the volumes devoted annually to scientific literature would be materially diminished in size. Were it applied, as far as the conditions of life admit, to the conduct and management of human affairs, we should not require to be told, when critical periods in our welfare as a nation arise, that we shall muddle through somehow. Recent experience has taught us that wise discretion and careful prevision are as necessary in the direction of public affairs as in the pursuit of science, and in both instances, when properly exercised, they enable us to reach with comparative certainty the goal which we strive to attain.

Improvements in Means of Observation.

Whilst certain principles of research are common to all the sciences, each great division requires for its investigation specialised arrangements to insure its progress. Nothing contributes so much to the advancement of knowledge as improvements in the means of observation, either by the discovery of new adjuncts to research, or by a fresh adaptation of old methods. In the industrial arts, the introduction of a new kind of raw material, the recognition that a mixture or blending is often more serviceable than when the substances employed are uncombined, the discovery of new processes of treating the articles used in manufactures, the invention of improved machinery, all lead to the expansion of trade, to the occupation of the people, and to the development of great industrial centres. In science, also, the invention and employment of new and more precise instruments and appliances enable us to appreciate more clearly the signification of facts and phenomena which were previously obscure, and to penetrate more deeply into the mysteries of nature. They mark fresh departures in the history of science, and provide a firm base of support from which a continuous advance may be made and fresh conceptions of nature can be evolved.

It is not my intention, even had I possessed the requisite knowledge, to undertake so arduous a task as to review the progress which has recently been made in the great body of sciences which lie within the domain of the British Association. As my occupation in life has required me to give attention to the science which deals with the structure and organisation of the bodies of man and animals—a science which either includes within its scope or has intimate and widespread relations to comparative anatomy, embryology, morphology, zoology, physiology, and anthropology—I shall limit myself to the attempt to bring before you some of the more important observations and conclusions which have a bearing on the present position of the subject. As this is the closing year of the century it will not, I think, be out of place to refer to the changes which a hundred years have brought about in our fundamental conceptions of the structure of animals. In science, as in business, it is well from time to time to take stock of what we have been doing, so that we may realise where we stand and ascertain the balance to our credit in the scientific ledger.

So far back as the time of the ancient Greeks it was known that the human body and those of the more highly organised animals were not homogeneous, but were built up of parts, the *partes dissimilares* (τὰ ἀνόμοια μέρη) of Aristotle, which differed from each other in form, colour, texture, consistency, and properties. These parts were familiarly known as the bones, muscles, sinews, blood-vessels, glands, brain, nerves, and so on. As the centuries rolled on, and as observers and observations multiplied, a more and more precise knowledge of these parts throughout the Animal Kingdom was obtained, and various attempts were made to classify animals in accordance with their forms and structure. During the concluding years of the last century and the earlier part of the present, the Hunters, William and John, in our country, the Meckels in Germany, Cuvier and St. Hilaire in France, gave an enormous impetus to anatomical studies, and contributed largely to our knowledge of the construction of the bodies of animals. But whilst by these and other observers the most salient and, if I may use the expression, the grosser characters of animal organisation had been recognised, little was known of the more intimate structure or texture of the parts. So far as could be determined by the unassisted vision, and so much as could be recognised by the use of a simple lens, had indeed been ascertained, and it was known that muscles, nerves, and tendons were composed of threads or fibres, that the blood- and lymph-vessels were tubes, that the parts which we call fasciæ and aponeuroses were thin membranes, and so on.

Early in the present century Xavier Bichat, one of the most brilliant men of science during the Napoleonic era in France, published his ‘Anatomie Générale,’ in which he formulated important general principles. Every animal is an assemblage of different organs, each of which discharges a function, and acting together, each in its own way, assists in the

preservation of the whole. The organs are, as it were, special machines situated in the general building which constitutes the factory or body of the individual. But, further, each organ or special machine is itself formed of tissues which possess different properties. Some, as the blood-vessels, nerves, fibrous tissues, &c., are generally distributed throughout the animal body, whilst others, as bones, muscles, cartilage, &c., are found only in certain definite localities. Whilst Bichat had acquired a definite philosophical conception of the general principles of construction and of the distribution of the tissues, neither he nor his pupil Béclard was in a position to determine the essential nature of the structural elements. The means and appliances at their disposal and at that of other observers in their generation were not sufficiently potent to complete the analysis.

Attempts were made in the third decennium of this century to improve the methods of examining minute objects by the manufacture of compound lenses, and, by doing away with chromatic and spherical aberration, to obtain, in addition to magnification of the object, a relatively large flat field of vision with clearness and sharpness of definition. When in January 1830 Joseph Jackson Lister read to the Royal Society his memoir 'On some properties in achromatic object-glasses applicable to the improvement of microscopes,' he announced the principles on which combinations of lenses could be arranged, which would possess these qualities. By the skill of our opticians, microscopes have now for more than half a century been constructed which, in the hands of competent observers, have influenced and extended biological science with results comparable to those obtained by the astronomer through improvements in the telescope.

In the study of the minute structure of plants and animals the observer has frequently to deal with tissues and organs, most of which possess such softness and delicacy of substance and outline that, even when microscopes of the best construction are employed, the determination of the intimate nature of the tissue, and the precise relation which one element of an organ bears to the other constituent elements, is in many instances a matter of difficulty. Hence additional methods have had to be devised in order to facilitate study and to give precision and accuracy to our observations. It is difficult for one of the younger generation of biologists, with all the appliances of a well-equipped laboratory at his command, with experienced teachers to direct him in his work, and with excellent text-books, in which the modern methods are described, to realise the conditions under which his predecessors worked half a century ago. Laboratories for minute biological research had not been constructed, the practical teaching of histology and embryology had not been organised, experience in methods of work had not accumulated; each man was left to his individual efforts, and had to puzzle his way through the complications of structure to the best of his power. Staining and hardening

reagents were unknown. The double-bladed knife invented by Valentin, held in the hand, was the only improvement on the scalpel or razor for cutting thin, more or less translucent slices suitable for microscopic examination; mechanical section-cutters and freezing arrangements had not been devised. The tools at the disposal of the microscopist were little more than knife, forceps, scissors, needles; with acetic acid, glycerine, and Canada balsam as reagents. But in the employment of the newer methods of research care has to be taken, more especially when hardening and staining reagents are used, to discriminate between appearances which are to be interpreted as indicating natural characters, and those which are only artificial productions.

Notwithstanding the difficulties attendant on the study of the more delicate tissues, the compound achromatic microscope provided anatomists with an instrument of great penetrative power. Between the years 1830 and 1850 a number of acute observers applied themselves with much energy and enthusiasm to the examination of the minute structure of the tissues and organs in plants and animals.

Cell Theory.

It had, indeed, long been recognised that the tissues of plants were to a large extent composed of minute vesicular bodies, technically called cells (Hooke, Malpighi, Grew). In 1831 the discovery was made by the great botanist, Robert Brown, that in many families of plants a circular spot, which he named areola or nucleus, was present in each cell; and in 1838 M. J. Schleiden published the fact that a similar spot or nucleus was a universal elementary organ in vegetables. In the tissues of animals also structures had begun to be recognised comparable with the cells and nuclei of the vegetable tissues, and in 1839 Theodore Schwann announced the important generalisation that there is one universal principle of development for the elementary part of organisms, however different they may be in appearance, and that this principle is the formation of cells. The enunciation of the fundamental principle that the elementary tissues consisted of cells constituted a step in the progress of biological science, which will for ever stamp the century now drawing to a close with a character and renown equalling those which it has derived from the most brilliant discoveries in the physical sciences. It provided biologists with the visible anatomical units through which the external forces operating on, and the energy generated in, living matter come into play. It dispelled for ever the old mystical idea of the influence exercised by vapours or spirits in living organisms. It supplied the physiologist and pathologist with the specific structures through the agency of which the functions of organisms are discharged in health and disease. It exerted an enormous influence on the progress of practical medicine. A review of the progress of knowledge of the cell may appropriately enter into an address on this occasion.

ADDRESS.

Structure of Cells.

A cell is a living particle, so minute that it needs a microscope for its examination ; it grows in size, maintains itself in a state of activity, responds to the action of stimuli, reproduces its kind, and in the course of time it degenerates and dies.

Let us glance at the structure of a cell to determine its constituent parts and the *rôle* which each plays in the function to be discharged. The original conception of a cell, based upon the study of the vegetable tissues, was a minute vesicle enclosed by a definite wall, which exercised chemical or metabolic changes on the surrounding material and secreted into the vesicle its characteristic contents. A similar conception was at first also entertained regarding the cells of animal tissues ; but as observations multiplied, it was seen that numerous elementary particles, which were obviously in their nature cells, did not possess an enclosing envelope. A wall ceased to have a primary value as a constituent part of a cell, the necessary vesicular character of which therefore could no longer be entertained.

The other constituent parts of a cell are the cell plasm, which forms the body of the cell, and the nucleus embedded in its substance. Notwithstanding the very minute size of the nucleus, which even in the largest cells is not more than $\frac{1}{1000}$ th inch in diameter, and usually is considerably smaller, its almost constant form, its well-defined sharp outline, and its power of resisting the action of strong reagents when applied to the cell, have from the period of its discovery by Robert Brown caused histologists to bestow on it much attention. Its structure and chemical composition ; its mode of origin ; the part which it plays in the formation of new cells, and its function in nutrition and secretion have been investigated.

When examined under favourable conditions in its passive or resting state, the nucleus is seen to be bounded by a membrane which separates it from the cell plasm and gives it the characteristic sharp contour. It contains an apparently structureless nuclear substance, nucleoplasm or enchylema, in which are embedded one or more extremely minute particles called nucleoli, along with a network of exceedingly fine threads or fibres, which in the active living cell play an essential part in the production of new nuclei within the cell. In its chemical composition the nuclear substance consists of albuminous plastin and globulin ; and of a special material named nuclein, rich in phosphorus and with an acid reaction. The delicate network within the nucleus consists apparently of the nuclein, a substance which stains with carmine and other dyes, a property which enables the changes, which take place in the network in the production of young cells, to be more readily seen and followed out by the observer.

The mode of origin of the nucleus and the part which it plays in the production of new cells have been the subject of much discussion

Schleiden, whose observations, published in 1838, were made on the cells of plants, believed that within the cell a nucleolus first appeared, and that around it molecules aggregated to form the nucleus. Schwann again, whose observations were mostly made on the cells of animals, considered that an amorphous material existed in organised bodies, which he called cytoblastema. It formed the contents of cells, or it might be situated free or external to them. He figuratively compared it to a mother liquor in which crystals are formed. Either in the cytoblastema within the cells or in that situated external to them, the aggregation of molecules around a nucleolus to form a nucleus might occur, and, when once the nucleus had been formed, in its turn it would serve as a centre of aggregation of additional molecules from which a new cell would be produced. He regarded therefore the formation of nuclei and cells as possible in two ways : one within pre-existing cells (endogenous cell-formation), the other in a free blastema lying external to cells (free cell-formation). In animals, he says, the endogenous method is rare, and the customary origin is in an external blastema. Both Schleiden and Schwann considered that after the cell was formed the nucleus had no permanent influence on the life of the cell, and usually disappeared.

Under the teaching principally of Henle, the famous Professor of Anatomy in Gottingen, the conception of the free formation of nuclei and cells in a more or less fluid blastema, by an aggregation of elementary granules and molecules, obtained so much credence, especially amongst those who were engaged in the study of pathological processes, that the origin of cells within pre-existing cells was to a large extent lost sight of. That a parent cell was requisite for the production of new cells seemed to many investigators to be no longer needed. Without doubt this conception of free cell-formation contributed in no small degree to the belief, entertained by various observers, that the simplest plants and animals might arise, without pre-existing parents, in organic fluids destitute of life, by a process of spontaneous generation ; a belief which prevailed in many minds almost to the present day. If, as has been stated, the doctrine of abiogenesis cannot be experimentally refuted, on the other hand it has not been experimentally proved. The burden of proof lies with those who hold the doctrine, and the evidence that we possess is all the other way.

Multiplication of Cells.

Although von Mohl, the botanist, seems to have been the first to recognise (1835) in plants a multiplication of cells by division, it was not until attention was given to the study of the egg in various animals, and to the changes which take place in it, attendant on fertilisation, that in the course of time a much more correct conception of the origin of the nucleus and of the part which it plays in the formation of new cells was obtained. Before Schwann had published his classical memoir, in 1839,

von Baer and other observers had recognised within the animal ovum the germinal vesicle, which obviously bore to the ovum the relation of a nucleus to a cell. As the methods of observation improved, it was recognised that, within the developing egg, two vesicles appeared where one only had previously existed, to be followed by four vesicles, then eight, and so on in multiple progression until the ovum contained a multitude of vesicles, each of which possessed a nucleus. The vesicles were obviously cells which had arisen within the original germ-cell or ovum. These changes were systematically described by Martin Barry so long ago as 1839 and 1840 in two memoirs communicated to the Royal Society of London, and the appearance produced, on account of the irregularities of the surface occasioned by the production of new vesicles, was named by him the mulberry-like structure. He further pointed out that the vesicles arranged themselves as a layer within the envelope of the egg or zona pellucida, and that the whole embryo was composed of cells filled with the foundations of other cells. He recognised that the new cells were derived from the germinal vesicle or nucleus of the ovum, the contents of which entered into the formation of the first two cells, each of which had its nucleus, which in its turn resolved itself into other cells, and by a repetition of the process into a greater number. The endogenous origin of new cells within a pre-existing cell and the process which we now term the segmentation of the yolk were successfully demonstrated. In a third memoir, published in 1841, Barry definitely stated that young cells originated through division of the nucleus of the parent cell, instead of arising, as a product of crystallisation, in the fluid cytotblastema of the parent cell or in a blastema situated external to the cell.

In a memoir published in 1842, John Goodsir advocated the view that the nucleus is the reproductive organ of the cell, and that from it, as from a germinal spot, new cells were formed. In a paper, published three years later, on nutritive centres, he described cells, the nuclei of which were the permanent source of successive broods of young cells, which from time to time occupied the cavity of the parent cell. He extended also his observations on the endogenous formation of cells to the cartilage cells in the process of inflammation and to other tissues undergoing pathological changes. Corroborative observations on endogenous formation were also given by his brother Harry Goodsir in 1845. These observations on the part which the nucleus plays by cleavage in the formation of young cells by endogenous development from a parent centre—that an organic continuity existed between a mother cell and its descendants through the nucleus—constituted a great step in advance of the views entertained by Schleiden and Schwann, and showed that Barry and the Goodsirs had a deeper insight into the nature and functions of cells than was possessed by most of their contemporaries, and are of the highest importance when viewed in the light of recent observations.

In 1841 Robert Remak published an account of the presence of two

nuclei in the blood corpuscles of the chick and the pig, which he regarded as evidence of the production of new corpuscles by division of the nucleus within a parent cell ; but it was not until some years afterwards (1850 to 1855) that he recorded additional observations and recognised that division of the nucleus was the starting-point for the multiplication of cells in the ovum and in the tissues generally. Remak's view was that the process of cell division began with the cleavage of the nucleolus, followed by that of the nucleus, and that again by cleavage of the body of the cell and of its membrane. Kölliker had previously, in 1843, described the multiplication of nuclei in the ova of parasitic worms, and drew the inference that in the formation of young cells within the egg the nucleus underwent cleavage, and that each of its divisions entered into the formation of a new cell. By these observations, and by others subsequently made, it became obvious that the multiplication of animal cells, either by division of the nucleus within the cell, or by the budding off of a part of the protoplasm of the cell, was to be regarded as a widely spread and probably a universal process, and that each new cell arose from a parent cell.

Pathological observers were, however, for the most part inclined to consider free cell-formation in a blastema or exudation by an aggregation of molecules, in accordance with the views of Henle, as a common phenomenon. This proposition was attacked with great energy by Virchow in a series of memoirs published in his 'Archiv,' commencing in Vol. 1, 1847, and finally received its death-blow in his published lectures on Cellular Pathology, 1858. He maintained that in pathological structures there was no instance of cell development *de novo* ; where a cell existed, there one must have been before. Cell-formation was a continuous development by descent, which he formulated in the expression *omnis cellula e cellula*.

Karyokinesis.

Whilst the descent of cells from pre-existing cells by division of the nucleus during the development of the egg, in the embryos of plants and animals, and in adult vegetable and animal tissues, both in healthy and diseased conditions, had now become generally recognised, the mechanism of the process by which the cleavage of the nucleus took place was for a long time unknown. The discovery had to be deferred until the optician had been able to construct lenses of a higher penetrative power, and the microscopist had learned the use of colouring agents capable of dyeing the finest elements of the tissues. There was reason to believe that in some cases a direct cleavage of the nucleus, to be followed by a corresponding division of the cell into two parts, did occur. In the period between 1870 and 1880 observations were made by Schneider, Strasburger, Bütschli, Fol, van Beneden, and Flemming, which showed that the division of the nucleus and the cell was due to a series of very remarkable changes, now known as indirect nuclear and cell division, or karyo-

kinesis. The changes within the nucleus are of so complex a character that it is impossible to follow them in detail without the use of appropriate illustrations. I shall have to content myself, therefore, with an elementary sketch of the process.

I have previously stated that the nucleus in its passive or resting stage contains a very delicate network of threads or fibres. The first stage in the process of nuclear division consists in the threads arranging themselves in loops and forming a compact coil within the nucleus. The coil then becomes looser, the loops of threads shorten and thicken, and somewhat later each looped thread splits longitudinally into two portions. As the threads stain when colouring agents are applied to them, they are called chromatin fibres, and the loose coil is the chromosome (Waldeyer).

As the process continues, the investing membrane of the nucleus disappears, and the loops of threads arrange themselves within the nucleus so that the closed ends of the loops are directed to a common centre, from which the loops radiate outwards and produce a starlike figure (aster). At the same time clusters of extremely delicate lines appear both in the nucleoplasm and in the body of the cell, named the achromatic figure, which has a spindle-like form with two opposite poles, and stains much more feebly than the chromatic fibres. The loops of the chromatic star then arrange themselves in the equatorial plane of the spindle, and bending round turn their closed ends towards the periphery of the nucleus and the cell.

The next stage marks an important step in the process of division of the nucleus. The two longitudinal portions, into which each looped thread had previously split, now separate from each other, and whilst one part migrates to one pole of the spindle, the other moves to the opposite pole, and the free ends of each loop are directed towards its equator (metakinesis). By this division of the chromatin fibres, and their separation from each other to opposite poles of the spindle, two star-like chromatin figures are produced (dyaster).

Each group of fibres thickens, shortens, becomes surrounded by a membrane, and forms a new or daughter nucleus (dispirem). Two nuclei therefore have arisen within the cell by the division of that which had previously existed, and the expression formulated by Flemming—*omnis nucleus e nucleo*—is justified. Whilst this stage is in course of being completed, the body of the cell becomes constricted in the equatorial plane of the spindle, and, as the constriction deepens, it separates into two parts, each containing a daughter nucleus, so that two nucleated cells have arisen out of a pre-existing cell.

A repetition of the process in each of these cells leads to the formation of other cells, and, although modifications in details are found in different species of plants and animals, the multiplication of cells in the egg and in the tissues generally on similar lines is now a thoroughly established fact in biological science.

In the study of karyokinesis, importance has been attached to the number of chromosomes in the nucleus of the cell. Flemming had seen in the Salamander twenty-four chromosome fibres, which seems to be a constant number in the cells of epithelium and connective tissues. In other cells again, especially in the ova of certain animals, the number is smaller, and fourteen, twelve, four, and even two only have been described. The theory formulated by Boveri that the number of chromosomes is constant for each species, and that in the karyokinetic figures corresponding numbers are found in homologous cells, seems to be not improbable.

In the preceding description I have incidentally referred to the appearance in the proliferating cell of an achromatic spindle-like figure. Although this was recognised by Fol in 1873, it is only during the last ten or twelve years that attention has been paid to its more minute arrangements and possible signification in cell-division.

The pole at each end of the spindle lies in the cell plasm which surrounds the nucleus. In the centre of each pole is a somewhat opaque spot (central body) surrounded by a clear space, which, along with the spot, constitutes the centrosome or the sphere of attraction. From each centrosome extremely delicate lines may be seen to radiate in two directions. One set extends towards the pole at the opposite end of the spindle, and, meeting or coming into close proximity with radiations from it, constitutes the body of the spindle, which, like a perforated mantle, forms an imperfect envelope around the nucleus during the process of division. The other set of radiations is called the polar, and extends in the region of the pole towards the periphery of the cell.

The question has been much discussed whether any constituent part of the achromatic figure, or the entire figure, exists in the cell as a permanent structure in its resting phase; or if it is only present during the process of karyokinesis. During the development of the egg the formation of young cells, by division of the segmentation nucleus, is so rapid and continuous that the achromatic figure, with the centrosome in the pole of the spindle, is a readily recognisable object in each cell. The polar and spindle-like radiations are in evidence during karyokinesis, and have apparently a temporary endurance and function. On the other hand, van Beneden and Boveri were of opinion that the central body of the centrosome did not disappear when the division of the nucleus came to an end, but that it remained as a constituent part of a cell lying in the cell plasm near to the nucleus. Flemming has seen the central body with its sphere in leucocytes, as well as in epithelial cells and those of other tissues. Subsequently Heidenhain and other histologists have recorded similar observations. It would seem, therefore, as if there were reason to regard the centrosome, like the nucleus, as a permanent constituent of a cell. This view, however, is not universally entertained. If not always capable of demonstration in the resting stage of a cell, it is doubtless to be regarded as potentially present, and ready to assume,

along with the radiations, a characteristic appearance when the process of nuclear division is about to begin.

One can scarcely regard the presence of so remarkable an appearance as the achromatic figure without associating with it an important function in the economy of the cell. As from the centrosome at the pole of the spindle both sets of radiations diverge, it is not unlikely that it acts as a centre or sphere of energy and attraction. By some observers the radiations are regarded as substantive fibrillar structures, elastic or even contractile in their properties. Others, again, look upon them as morphological expressions of chemical and dynamical energy in the protoplasm of the cell body. On either theory we may assume that they indicate an influence, emanating, it may be, from the centrosome, and capable of being exercised both on the cell plasm and on the nucleus contained in it. On the contractile theory, the radiations which form the body of the spindle, either by actual traction of the supposed fibrillæ or by their pressure on the nucleus which they surround, might impel during karyokinesis the dividing chromosome elements towards the poles of the spindle, to form there the daughter nuclei. On the dynamical theory, the chemical and physical energy in the centrosome might influence the cell plasm and the nucleus, and attract the chromosome elements of the nucleus to the poles of the spindle. The radiated appearance would therefore be consequent and attendant on the physico-chemical activity of the centrosome. One or other of these theories may also be applied to the interpretation of the significance of the polar radiations.

Cell Plasm.

In the cells of plants, in addition to the cell wall, the cell body and the cell juice require to be examined. The material of the cell body, or the cell contents, was named by von Mohl (1846) protoplasm, and consisted of a colourless tenacious substance which partly lined the cell wall (primordial utricle), and partly traversed the interior of the cell as delicate threads enclosing spaces (vacuoles) in which the cell juice was contained. In the protoplasm the nucleus was embedded. Nägeli, about the same time, had also recognised the difference between the protoplasm and the other contents of vegetable cells, and had noticed its nitrogenous composition.

Though the analogy with a closed bladder or vesicle could no longer be sustained in the animal tissues, the name 'cell' continued to be retained for descriptive purposes, and the body of the cell was spoken of as a more or less soft substance enclosing a nucleus (Leydig). In 1861 Max Schultze adopted for the substance forming the body of the animal cell the term 'protoplasm.' He defined a cell to be a particle of protoplasm in the substance of which a nucleus was situated. He regarded the protoplasm, as indeed had previously been pointed out by the botanist

Unger, as essentially the same as the contractile sarcode which constitutes the body and pseudopodia of the *Amoeba* and other *Rhizopoda*. As the term 'protoplasm,' as well as that of 'bioplasm' employed by Lionel Beale in a somewhat similar though not precisely identical sense, involves certain theoretical views of the origin and function of the body of the cell, it would be better to apply to it the more purely descriptive term 'cytoplasm' or 'cell plasm.'

Schultze defined protoplasm as a homogeneous, glassy, tenacious material, of a jelly-like or somewhat firmer consistency, in which numerous minute granules were embedded. He regarded it as the part of the cell especially endowed with vital energy, whilst the exact function of the nucleus could not be defined. Based upon this conception of the jelly-like character of protoplasm, the idea for a time prevailed that a structureless, dimly granular, jelly or slime destitute of organisation, possessed great physiological activity, and was the medium through which the phenomena of life were displayed.

More accurate conceptions of the nature of the cell plasm soon began to be entertained. Brücke that the body of the cell was not simple, but had a complex organisation. Flemming observed that the cell plasm contained extremely delicate threads, which frequently formed a network, the interspaces of which were occupied by a more homogeneous substance. Where the threads crossed each other, granular particles (mikrosomen) were situated. Bütschli considered that he could recognise in the cell plasm a honeycomb-like appearance, as if it consisted of excessively minute chambers in which a homogeneous more or less fluid material was contained. The polar and spindle-like radiations visible during the process of karyokinesis, which have already been referred to, and the presence of the centrosome, possibly even during the resting stage of the cell, furnished additional illustrations of differentiation within the cell plasm. In many cells there appears also to be a difference in the character of the cell plasm which immediately surrounds the nucleus and that which lies at and near the periphery of the cell. The peripheral part (ektoplasma) is more compact and gives a definite outline to the cell, although not necessarily differentiating into a cell membrane. The inner part (endoplasma) is softer, and is distinguished by a more distinct granular appearance, and by containing the products specially formed in each particular kind of cell during the nutritive process.

By the researches of numerous investigators on the internal organisation of cells in plants and animals, a large body of evidence has now been accumulated, which shows that both the nucleus and the cell plasm consist of something more than a homogeneous, more or less viscid, slimy material. Recognisable objects in the form of granules, threads, or fibres can be distinguished in each. The cell plasm and the nucleus respectively are therefore not of the same constitution throughout, but possess polymorphic characters, the study of which in health and the changes produced by disease will for many years to come form important matters for investigation.

Function of Cells.

It has already been stated that, when new cells arise within pre-existing cells, division of the nucleus is associated with cleavage of the cell plasm, so that it participates in the process of new cell-formation. Undoubtedly, however, its rôle is not limited to this function. It also plays an important part in secretion, nutrition, and the special functions discharged by the cells in the tissues and organs of which they form morphological elements.

Between 1838 and 1842 observations were made which showed that cells were constituent parts of secreting glands and mucous membranes (Schwann, Henle). In 1842 John Goodsir communicated to the Royal Society of Edinburgh a memoir on secreting structures, in which he established the principle that cells are the ultimate secreting agents; he recognised in the cells of the liver, kidney, and other organs the characteristic secretion of each gland. The secretion was, he said, situated between the nucleus and the cell wall. At first he thought that, as the nucleus was the reproductive organ of the cell, the secretion was formed in the interior of the cell by the agency of the cell wall; but three years later he regarded it as a product of the nucleus. The study of the process of spermatogenesis by his brother, Harry Goodsir, in which the head of the spermatozoon was found to correspond with the nucleus of the cell in which the spermatozoon arose, gave support to the view that the nucleus played an important part in the genesis of the characteristic product of the gland cell.

The physiological activity of the cell plasm and its complex chemical constitution soon after began to be recognised. Some years before Max Schultze had published his memoirs on the characters of protoplasm, Brücke had shown that the well-known changes in tint in the skin of the *Chamæleon* were due to pigment granules situated in cells in the skin which were sometimes diffused throughout the cells, at others concentrated in the centre. Similar observations on the skin of the frog were made in 1854 by von Wittich and Harless. The movements were regarded as due to contraction of the cell wall on its contents. In a most interesting paper on the pigmentary system in the frog, published in 1858, Lord Lister demonstrated that the pigment granules moved in the cell plasma, by forces resident within the cell itself, acting under the influence of an external stimulant, and not by a contractility of the wall. Under some conditions the pigment was attracted to the centre of the cell, when the skin became pale; under other conditions the pigment was diffused throughout the body and the branches of the cell, and gave to the skin a dark colour. It was also experimentally shown that a potent influence over these movements was exercised by the nervous system.

The study of the cells of glands engaged in secretion, even when the

secretion is colourless, and the comparison of their appearance when secretion is going on with that seen when the cells are at rest, have shown that the cell plasm is much more granular and opaque, and contains larger particles during activity than when the cell is passive; the body of the cell swells out from an increase in the contents of its plasm, and chemical changes accompany the act of secretion. Ample evidence, therefore, is at hand to support the position taken by John Goodsir, nearly sixty years ago, that secretions are formed within cells, and lie in that part of the cell which we now say consists of the cell plasm; that each secreting cell is endowed with its own peculiar property, according to the organ in which it is situated, so that bile is formed by the cells in the liver, milk by those in the mamma, and so on.

Intimately associated with the process of secretion is that of nutrition. As the cell plasm lies at the periphery of a cell, and as it is, alike both in secretion and nutrition, brought into closest relation with the surrounding medium, from which the pabulum is derived, it is necessarily associated with nutritive activity. Its position enables it to absorb nutritive material directly from without, and in the process of growth it increases in amount by interstitial changes and additions throughout its substance, and not by mere accretions on its surface.

Hitherto I have spoken of a cell as a unit, independent of its neighbours as regards its nutrition and the other functions which it has to discharge. The question has, however, been discussed, whether in a tissue composed of cells closely packed together cell plasm may not give origin to processes or threads which are in contact or continuous with corresponding processes of adjoining cells, and that cells may therefore, to some extent, lose their individuality in the colony of which they are members. Appearances were recognised between 1863 and 1870 by Schron and others in the deeper cells of the epidermis and of some mucous membranes which gave sanction to this view, and it seems possible through contact or continuity of threads connecting a cell with its neighbours, that cells may exercise a direct influence on each other.

Nägeli, the botanist, as the foundation of a mechanico-physiological theory of descent, considered that in plants a network of cell plasm, named by him *idio-plasm*, extended throughout the whole of the plant, forming its specific molecular constitution, and that growth and activity were regulated by its conditions of tension and movements (1884).

The study of the structure of plants with special reference to the presence of an intercellular network has for some years been pursued by Walter Gardiner (1882-97), who has demonstrated threads of cell plasm protruding through the walls of vegetable cells and continuous with similar threads from adjoining cells. Structurally, therefore, a plant may be conceived to be built up of a nucleated cytoplasmic network, each nucleus with the branching cell plasm surrounding it being a centre of activity. On this view a cell would retain to some extent its individuality,

though, as Gardiner contends, the connecting threads would be the medium for the conduction of impulses and of food from a cell to those which lie around it. For the plant cell therefore, as has long been accepted in the animal cell, the wall is reduced to a secondary position, and the active constituent is the nucleated cell plasm. It is not unlikely that the absence of a controlling nervous system in plants requires the plasm of adjoining cells to be brought into more immediate contact and continuity than is the case with the generality of animal cells, so as to provide a mechanism for harmonising the nutritive and other functional processes in the different areas in the body of the plant. In this particular, it is of interest to note that the epithelial tissues in animals, where somewhat similar connecting arrangements occur, are only indirectly associated with the nervous and vascular systems, so that, as in plants, the cells may require, for nutritive and other purposes, to act and react directly on each other.

Nerve Cells.

Of recent years great attention has been paid to the intimate structure of nerve cells, and to the appearance which they present when in the exercise of their functional activity. A nerve cell is not a secreting cell; that is, it does not derive from the blood or surrounding fluid a pabulum which it elaborates into a visible, palpable secretion characteristic of the organ of which the cell is a constituent element, to be in due course discharged into a duct which conveys the secretion out of the gland. Nerve cells, through the metabolic changes which take place in them in connection with their nutrition, are associated with the production of the form of energy specially exhibited by animals which possess a nervous system, termed nerve energy. It has long been known that every nerve cell has a body in which a relatively large nucleus is situated. A most important discovery was the recognition that the body of every nerve cell had one or more processes growing out from it. More recently it has been proved, chiefly through the researches of Schultze, His, Golgi, and Ramon y Cajal, that at least one of the processes, the axon of the nerve cell, is continued into the axial cylinder of a nerve fibre, and that in the multipolar nerve cell the other processes, or dendrites, branch and ramify for some distance away from the body. A nerve fibre is therefore an essential part of the cell with which it is continuous, and the cell, its processes, the nerve fibre and the collaterals which arise from the nerve fibre collectively form a neuron or structural nerve unit (Waldeyer). The nucleated body of the nerve cell is the physiological centre of the unit.

The cell plasm occupies both the body of the nerve cell and its processes. The intimate structure of the plasm has, by improved methods of observation introduced during the last eight years by Nissl, and conducted on similar lines by other investigators, become more definitely understood. It has been ascertained that it possesses two distinct

characters which imply different structures. One of these stains deeply on the addition of certain dyes, and is named chromophile or chromatic substance ; the other, which does not possess a similar property, is the achromatic network. The chromophile is found in the cell body and the dendritic processes, but not in the axon. It occurs in the form of granular particles, which may be scattered throughout the plasm, or aggregated into little heaps which are elongated or fusiform in shape and appear as distinct coloured particles or masses. The achromatic network is found in the cell body and the dendrites, and is continued also into the axon, where it forms the axial cylinder of the nerve fibre. It consists apparently of delicate threads or fibrillæ, in the meshes of which a homogeneous material, such as is found in cell plasm generally, is contained. In the nerve cells, as in other cells, the plasm is without doubt concerned in the process of cell nutrition. The achromatic fibrillæ exercise an important influence on the axon or nerve fibre with which they are continuous, and probably they conduct the nerve impulses which manifest themselves in the form of nerve energy. The dendritic processes of a multipolar nerve cell ramify in close relation with similar processes branching from other cells in the same group. The collaterals and the free end of the axon fibre process branch and ramify in association with the body of a nerve cell or of its dendrites. We cannot say that these parts are directly continuous with each other to form an intercellular network, but they are apparently in apposition, and through contact exercise influence one on the other in the transmission of nerve impulses.

There is evidence to show that in the nerve cell the nucleus, as well as the cell plasm, is an effective agent in nutrition. When the cell is functionally active, both the cell body and the nucleus increase in size (Vas, G. Mann, Lugaro) ; on the other hand, when nerve cells are fatigued through excessive use, the nucleus decreases in size and shrivels ; the cell plasm also shrinks, and its coloured or chromophile constituent becomes diminished in quantity, as if it had been consumed during the prolonged use of the cell (Hodge, Mann, Lugaro). It is interesting also to note that in hibernating animals in the winter season, when their functional activity is reduced to a minimum, the chromophile in the plasm of the nerve cells is much smaller in amount than when the animal is leading an active life in the spring and summer (G. Levi).

When a nerve cell has attained its normal size it does not seem to be capable of reproducing new cells in its substance by a process of karyokinesis, such as takes place when young cells arise in the egg and in the tissues generally. It would appear that nerve cells are so highly specialised in their association with the evolution of nerve energy, that they have ceased to have the power of reproducing their kind, and the metabolic changes both in cell plasm and nucleus are needed to enable them to discharge their very peculiar function. Hence it follows that when a portion of the brain or other nerve-centre is destroyed, the

injury is not repaired by the production of fresh specimens of their characteristic cells, as would be the case in injuries to bones and tendons.

In our endeavours to differentiate the function of the nucleus from that of the cell plasm, we should not regard the former as concerned only in the production of young cells, and the latter as the exclusive agent in growth, nutrition, and, where gland cells are concerned, in the formation of their characteristic products. As regards cell reproduction also, though the process of division begins in the nucleus in its chromosome constituents, the achromatic figure in the cell plasm undoubtedly plays a part, and the cell plasm itself ultimately undergoes cleavage.

A few years ago the tendency amongst biologists was to ignore or attach but little importance to the physiological use of the nucleus in the nucleated cell, and to regard the protoplasm as the essential and active constituent of living matter; so much so, indeed, was this the case that independent organisms regarded as distinct species were described as consisting of protoplasm destitute of a nucleus; also that scraps of protoplasm separated from larger nucleated masses could, when isolated, exhibit vital phenomena. There is reason to believe that a fragment of protoplasm, when isolated from the nucleus of a cell, though retaining its contractility and capable of nourishing itself for a short time, cannot increase in amount, act as a secreting structure, or reproduce its kind: it soon loses its activity, withers, and dies. In order that these qualities of living matter should be retained, a nucleus is by most observers regarded as necessary (Nussbaum, Gruber, Haberlandt, Korschelt), and for the complete manifestation of vital activity both nucleus and cell plasm are required.

Bacteria.

The observations of Cohn, made about thirty years ago, and those of De Bary shortly afterwards, brought into notice a group of organisms to which the name 'bacterium' or 'microbe' is given. They were seen to vary in shape: some were rounded specks called cocci, others were straight rods called bacilli, others were curved or spiral rods, vibrios or spirillæ. All were characterised by their extreme minuteness, and required for their examination the highest powers of the best microscopes. Many bacteria measure in their least diameter not more than $\frac{1}{25000}$ th of an inch, $\frac{1}{10}$ th the diameter of a human white blood corpuscle. Through the researches of Pasteur, Lord Lister, Koch, and other observers, bacteria have been shown to play an important part in nature. They exercise a very remarkable power over organic substances, especially those which are complex in chemical constitution, and can resolve them into simpler combinations. Owing to this property, some bacteria are of great economic value, and without their agency many of our industries could not be pursued; others again, and these are the most talked of, exercise a malignant influence in the production of the most deadly diseases which afflict man and the domestic animals.

Great attention has been given to the structure of bacteria and to their mode of propagation. When examined in the living state and magnified about 2,000 times, a bacterium appears as a homogeneous particle, with a sharp definite outline, though a membranous envelope or wall, distinct from the body of the bacterium, cannot at first be recognised; but when treated with reagents a membranous envelope appears, the presence of which, without doubt, gives precision of form to the bacterium. The substance within the membrane contains granules which can be dyed with colouring agents. Owing to their extreme minuteness it is difficult to pronounce an opinion on the nature of the chromatine granules and the substance in which they lie. Some observers regard them as nuclear material, invested by only a thin layer of protoplasm, on which view a bacterium would be a nucleated cell. Others consider the bacterium as formed of protoplasm containing granules capable of being coloured, which are a part of the protoplasm itself, and not a nuclear substance. On the latter view, bacteria would consist of cell plasma enclosed in a membrane and destitute of a nucleus. Whatever be the nature of the granule-containing material, each bacterium is regarded as a cell, the minutest and simplest living particle capable of an independent existence that has yet been discovered.

Bacteria cells, like cells generally, can reproduce their kind. They multiply by simple fission, probably with an ingrowth of the cell wall, but without the karyokinetic phenomena observed in nucleated cells. Each cell gives rise to two daughter cells, which may for a time remain attached to each other and form a cluster or a chain, or they may separate and become independent isolated cells. The multiplication, under favourable conditions of light, air, temperature, moisture, and food, goes on with extraordinary rapidity, so that in a few hours many thousand new individuals may arise from a parent bacterium.

Connected with the life-history of a bacterium cell is the formation in its substance, in many species and under certain conditions, of a highly refractile shiny particle called a spore. At first sight a spore seems as if it were the nucleus of the bacterium cell, but it is not always present when multiplication by cleavage is taking place, and when present it does not appear to take part in the fission. On the other hand, a spore, from the character of its envelope, possesses great power of resistance, so that dried bacteria, when placed in conditions favourable to germination, can through their spores germinate and resume an active existence. Spore formation seems, therefore, to be a provision for continuing the life of the bacterium under conditions which, if spores had not formed, would have been the cause of its death.

The time has gone by to search for the origin of living organisms by a spontaneous aggregation of molecules in vegetable or other infusions, or from a layer of formless primordial slime diffused over the bed of the ocean. Living matter during our epoch has been, and continues to be, derived

from pre-existing living matter, even when it possesses the simplicity of structure of a bacterium, and the morphological unit is the cell.

Development of the Egg.

As the future of the entire organism lies in the fertilised egg cell, we may now briefly review the arrangements, consequent on the process of segmentation, which lead to the formation, let us say in the egg of a bird, of the embryo or young chick.

In the latter part of the last century, C. F. Wolff observed that the beginning of the embryo was associated with the formation of layers, and in 1817 Pander demonstrated that in the hen's egg at first one layer, called mucous, appeared, then a second or serous layer, to be followed by a third, intermediate or vascular layer. In 1828 von Baer amplified our knowledge in his famous treatise, which from its grasp of the subject created a new epoch in the science of embryology. It was not, however, until the discovery by Schwann of cells as constant factors in the structure of animals and in their relation to development that the true nature of these layers was determined. We now know that each layer consists of cells, and that all the tissues and organs of the body are derived from them. Numerous observers have devoted themselves for many years to the study of each layer, with the view of determining the part which it takes in the formation of the constituent parts of the body, more especially in the higher animals, and the important conclusion has been arrived at that each kind of tissue invariably arises from one of these layers and from no other.

The layer of cells which contributes, both as regards the number and variety of the tissues derived from it, most largely to the formation of the body is the middle layer or mesoblast. From it the skeleton, the muscles, and other locomotor organs, the true skin, the vascular system, including the blood, and other structures which I need not detail, take their rise. From the inner layer of cells or hypoblast, the principal derivatives are the epithelial lining of the alimentary canal and of the glands which open into it, and the epithelial lining of the air-passages. The outer or epiblast layer of cells gives origin to the epidermis or scarf skin and to the nervous system. It is interesting to note that from the same layer of the embryo arise parts so different in importance as the cuticle—a mere protecting structure, which is constantly being shed when the skin is subjected to the friction of a towel or the clothes—and the nervous system, including the brain, the most highly differentiated system in the animal body. How completely the cells from which they are derived had diverged from each other in the course of their differentiation in structure and properties is shown by the fact that the cells of the epidermis are continually engaged in reproducing new cells to replace those which are shed, whilst the cells of the nervous system have apparently lost the power of reproducing their kind.

In the early stage of the development of the egg, the cells in a given layer resemble each other in form, and, as far as can be judged from their appearance, are alike in structure and properties. As the development proceeds, the cells begin to show differences in character, and in the course of time the tissues which arise in each layer differentiate from each other and can be readily recognised by the observer. To use the language of von Baer, a generalised structure has become specialised, and each of the special tissues produced exhibits its own structure and properties. These changes are coincident with a rapid multiplication of the cells by cleavage, and thus increase in size of the embryo accompanies specialisation of structure. As the process continues, the embryo gradually assumes the shape characteristic of the species to which its parents belonged, until at length it is fit to be born and to assume a separate existence.

The conversion of cells, at first uniform in character, into tissues of a diverse kind is due to forces inherent in the cells in each layer. The cell plasm plays an active though not an exclusive part in the specialisation; for as the nucleus influences nutrition and secretion, it acts as a factor in the differentiation of the tissues. When tissues so diverse in character as muscular fibre, cartilage, fibrous tissues, and bone arise from the cells of the middle or mesoblast layer, it is obvious that, in addition to the morphological differentiation affecting form and structure, a chemical differentiation affecting composition also occurs, as the result of which a physiological differentiation takes place. The tissues and organs become fitted to transform the energy derived from the food into muscular energy, nerve energy, and other forms of vital activity. Corresponding differentiations also modify the cells of the outer and inner layers. Hence the study of the development of the generalised cell layers in the young embryo enables us to realise how all the complex constituent parts of the body in the higher animals and in man are evolved by the process of differentiation from a simple nucleated cell—the fertilised ovum. A knowledge of the cell and of its life-history is therefore the foundation-stone on which biological science in all its departments is based.

If we are to understand by an organ in the biological sense a complex body capable of carrying on a natural process, a nucleated cell is an organ in its simplest form. In a unicellular animal or plant such an organ exists in its most primitive stage. The higher plants and animals again are built up of multitudes of these organs, each of which, whilst having its independent life, is associated with the others, so that the whole may act in unison for a common purpose. As in one of your great factories each spindle is engaged in twisting and winding its own thread, it is at the same time intimately associated with the hundreds of other spindles in its immediate proximity, in the manufacture of the yarn from which the web of cloth is ultimately to be woven.

It has taken more than fifty years of hard and continuous work to bring our knowledge of the structure and development of the tissues and

organs of plants and animals up to the level of the present day. Amidst the host of names of investigators, both at home and abroad, who have contributed to its progress, it may seem invidious to particularise individuals. There are, however, a few that I cannot forbear to mention, whose claim to be named on such an occasion as this will be generally conceded.

Botanists will, I think, acknowledge Wilhelm Hofmeister as a master in morphology and embryology, Julius von Sachs as the most important investigator in vegetable physiology during the last quarter of a century, and Strasburger as a leader in the study of the phenomena of nuclear division.

The researches of the veteran Professor of Anatomy in Würzburg, Albert von Kolliker, have covered the entire field of animal histology. His first paper, published fifty-nine years ago, was followed by a succession of memoirs and books on human and comparative histology and embryology, and culminated in his great treatise on the structure of the brain, published in 1896. Notwithstanding the weight of more than eighty years, he continues to prosecute histological research, and has published the results of his latest, though let us hope not his last, work during the present year.

Amongst our own countrymen, and belonging to the generation which has almost passed away, was William Bowman. His investigations between 1840 and 1850 on the mucous membranes, muscular fibre, and the structure of the kidney, together with his researches on the organs of sense, were characterised by a power of observation and of interpreting difficult and complicated appearances which has made his memoirs on these subjects landmarks in the history of histological inquiry.

Of the younger generation of biologists Francis Maitland Balfour, whose early death is deeply deplored as a loss to British science, was one of the most distinguished. His powers of observation and philosophic perception gave him a high place as an original inquirer, and the charm of his personality—for charm is not the exclusive possession of the fairer sex—endured him to his friends.

General Morphology.

Along with the study of the origin and structure of the tissues of organised bodies, much attention has been given during the century to the parts or organs in plants and animals, with the view of determining where and how they take their rise, the order of their formation, the changes which they pass through in the early stages of development, and their relative positions in the organism to which they belong. Investigations on these lines are spoken of as morphological, and are to be distinguished from the study of their physiological or functional relations, though both are necessary for the full comprehension of the living organism.

The first to recognise that morphological relations might exist between the organs of a plant, dissimilar as regards their function, was the poet Goethe, whose observations, guided by his imaginative faculty, led him to declare that the calyx, corolla, and other parts of a flower, the scales of a bulb, &c., were metamorphosed leaves, a principle generally accepted by botanists, and indeed extended to other parts of a plant, which are referred to certain common morphological forms although they exercise different functions. Goethe also applied the same principle in the study of the skeletons of vertebrate animals, and he formed the opinion that the spinal column and the skull were essentially alike in construction, and consisted of vertebræ, an idea which was also independently conceived and advocated by Oken.

The anatomist who in our country most strenuously applied himself to the morphological study of the skeleton was Richard Owen, whose knowledge of animal structure, based upon his own dissections, was unrivalled in range and variety. He elaborated the conception of an ideal, archetype vertebrate form which had no existence in nature, and to which, subject to modifications in various directions, he considered all vertebrate skeletons might be referred. Owen's observations were conducted to a large extent on the skeletons of adult animals, of the knowledge of which he was a master. As in the course of development modifications in shape and in the relative position of parts not unfrequently occur and their original character and place of origin become obscured, it is difficult, from the study only of adults, to arrive at a correct interpretation of their morphological significance. When the changes which take place in the skull during its development, as worked out by Reichert and Rathke, became known and their value had become appreciated, many of the conclusions arrived at by Owen were challenged and ceased to be accepted. It is, however, due to that eminent anatomist to state from my personal knowledge of the condition of anatomical science in this country fifty years ago, that an enormous impulse was given to the study of comparative morphology by his writings, and by the criticisms to which they were subjected.

There can be no doubt that generalised arrangements do exist in the early embryo which, up to a certain stage, are common to animals that in their adult condition present diverse characters, and out of which the forms special to different groups are evolved. As an illustration of this principle, I may refer to the stages of development of the great arteries in the bodies of vertebrate animals. Originally, as the observations of Rathke have taught us, the main arteries are represented by pairs of symmetrically arranged vascular arches, some of which enlarge and constitute the permanent arteries in the adult, whilst others disappear. The increase in size of some of these arches, and the atrophy of others, are so constant for different groups that they constitute anatomical features as distinctive as the modifications in the skeleton itself. Thus in mammals the fourth vascular arch on the left side persists, and forms the arch

of the aorta ; in birds the corresponding part of the aorta is an enlargement of the fourth right arch, and in reptiles both arches persist to form the great artery. That this original symmetry exists also in man we know from the fact that now and again his body, instead of corresponding with the mammalian type, has an aortic arch like that which is natural to the bird, and in rarer cases even to the reptile. A type form common to the vertebrata does therefore in such cases exist, capable of evolution in more than one direction.

The reputation of Thomas Henry Huxley as a philosophic comparative anatomist rests largely on his early perception of, and insistence on, the necessity of testing morphological conclusions by a reference to the development of parts and organs, and by applying this principle in his own investigations. The principle is now so generally accepted by both botanists and anatomists that morphological definitions are regarded as depending essentially on the successive phases of the development of the parts under consideration.

The morphological characters exhibited by a plant or animal tend to be hereditarily transmitted from parents to offspring, and the species is perpetuated. In each species the evolution of an individual, through the developmental changes in the egg, follows the same lines in all the individuals of the same species, which possess therefore in common the features called specific characters. The transmission of these characters is due, according to the theory of Weismann, to certain properties possessed by the chromosome constituents of the segmentation nucleus in the fertilised ovum, named by him the germ plasm, which is continued from one generation to another, and impresses its specific character on the egg and on the plant or animal developed from it.

As has already been stated, the special tissues which build up the bodies of the more complex organisms are evolved out of cells which are at first simple in form and appearance. During the evolution of the individual, cells become modified or differentiated in structure and function, and so long as the differentiation follows certain prescribed lines the morphological characters of the species are preserved. We can readily conceive that, as the process of specialisation is going on, modifications or variations in groups of cells and the tissues derived from them, notwithstanding the influence of heredity, may in an individual diverge so far from that which is characteristic of the species as to assume the arrangements found in another species, or even in another order. Anatomists had indeed long recognised that variations from the customary arrangement of parts occasionally appeared, and they described such deviations from the current descriptions as irregularities.

Darwinian Theory.

The signification of the variations which arise in plants and animals had not been apprehended until a flood of light was thrown on the entire

subject by the genius of Charles Darwin, who formulated the wide-reaching theory that variations could be transmitted by heredity to younger generations. In this manner he conceived new characters would arise, accumulate, and be perpetuated, which would in the course of time assume specific importance. New species might thus be evolved out of organisms originally distinct from them, and their specific characters would in turn be transmitted to their descendants. By a continuance of this process new species would multiply in many directions, until at length from one or more originally simple forms the earth would become peopled by the infinite varieties of plant and animal organisms which have in past ages inhabited, or do at present inhabit, our globe. The Darwinian theory may therefore be defined as Heredity modified and influenced by Variability. It assumes that there is an heredity quality in the egg which, if we take the common fowl for an example, shall continue to produce similar fowls. Under conditions, of which we are ignorant, which occasion molecular changes in the cells and tissues of the developing egg, variations might arise, in the first instance probably slight, but becoming intensified in successive generations, until at length the descendants would have lost the characters of the fowl and have become another species. No precise estimate has been arrived at, and indeed one does not see how it is possible to obtain it, of the length of years which might be required to convert a variation, capable of being transmitted, into a new and definite specific character.

The circumstances which, according to the Darwinian theory, determined the perpetuation by hereditary transmission of a variety and its assumption of a specific character depended, it was argued, on whether it possessed such properties as enabled the plant or animal in which it appeared to adapt itself more readily to its environment, *i.e.* to the surrounding conditions. If it were to be of use the organism in so far became better adapted to hold its own in the struggle for existence with its fellows and with the forces of nature operating on it. Through the accumulation of useful characters the specific variety was perpetuated by natural selection so long as the conditions were favourable for its existence, and it survived as being the best fitted to live. In the study of the transmission of variations which may arise in the course of development it should not be too exclusively thought that only those variations are likely to be preserved which can be of service during the life of the individual, or in the perpetuation of the species, and possibly available for the evolution of new species. It should also be kept in mind that morphological characters can be transmitted by hereditary descent, which, though doubtless of service in some bygone ancestor, are in the new conditions of life of the species of no physiological value. Our knowledge of the structural and functional modifications to be found in the human body, in connection with abnormalities and with tendencies or predisposition to diseases of various kinds, teaches us that

characters which are of no use, and indeed detrimental to the individual, may be hereditarily transmitted from parents to offspring through a succession of generations.

Since the conception of the possibility of the evolution of new species from pre-existing forms took possession of the minds of naturalists, attempts have been made to trace out the lines on which it has proceeded. The first to give a systematic account of what he conceived to be the order of succession in the evolution of animals was Ernst Haeckel, of Jena, in a well-known treatise. Memoirs on special departments of the subject, too numerous to particularise, have subsequently appeared. The problem has been attacked along two different lines: the one by embryologists, of whom may be named Kowalewsky, Gegenbaur, Dohrn, Ray Lankester, Balfour, and Gaskell, who with many others have conducted careful and methodical inquiries into the stages of development of numerous forms belonging to the two great divisions of the animal kingdom. Invertebrates, as well as vertebrates, have been carefully compared with each other in the bearing of their development and structure on their affinities and descent, and the possible sequence in the evolution of the Vertebrata from the Invertebrata has been discussed. The other method pursued by palæontologists, of whom Huxley, Marsh, Cope, Osborne, and Traquair are prominent authorities, has been the study of the extinct forms preserved in the rocks and the comparison of their structure with each other and with that of existing organisms. In the attempts to trace the line of descent the imagination has not unfrequently been called into play in constructing various conflicting hypotheses. Though from the nature of things the order of descent is, and without doubt will continue to be, ever a matter of speculation and not of demonstration, the study of the subject has been a valuable intellectual exercise and a powerful stimulant to research.

We know not as regards time when the fiat went forth, 'Let there be Life, and there was Life.' All we can say is that it must have been in the far-distant past, at a period so remote from the present that the mind fails to grasp the duration of the interval. Prior to its genesis our earth consisted of barren rock and desolate ocean. When matter became endowed with Life, with the capacity of self-maintenance and of resisting external disintegrating forces, the face of nature began to undergo a momentous change. Living organisms multiplied, the land became covered with vegetation, and multitudinous varieties of plants, from the humble fungus and moss to the stately palm and oak, beautified its surface and fitted it to sustain higher kinds of living beings. Animal forms appeared, in the first instance simple in structure, to be followed by others more complex, until the mammalian type was produced. The ocean also became peopled with plant and animal organisms, from the microscopic diatom to the huge leviathan. Plants and animals acted and reacted on each other, on the atmosphere which surrounded them and on

the earth on which they dwelt, the surface of which became modified in character and aspect. At last Man came into existence. His nerve-energy, in addition to regulating the processes in his economy which he possesses in common with animals, was endowed with higher powers. When translated into psychical activity it has enabled him throughout the ages to progress from the condition of a rude savage to an advanced stage of civilisation ; to produce works in literature, art, and the moral sciences which have exerted, and must continue to exert, a lasting influence on the development of his higher Being ; to make discoveries in physical science ; to acquire a knowledge of the structure of the earth, of the ocean in its changing aspects, of the atmosphere and the stellar universe, of the chemical composition and physical properties of matter in its various forms, and to analyse, comprehend, and subdue the forces of nature.

By the application of these discoveries to his own purposes Man has, to a large extent, overcome time and space ; he has studded the ocean with steamships, girdled the earth with the electric wire, tunnelled the lofty Alps, spanned the Forth with a bridge of steel, invented machines and founded industries of all kinds for the promotion of his material welfare, elaborated systems of government fitted for the management of great communities, formulated economic principles, obtained an insight into the laws of health, the causes of infective diseases, and the means of controlling and preventing them.

When we reflect that many of the most important discoveries in abstract science and in its applications have been made during the present century, and indeed since the British Association held its first meeting in the ancient capital of your county sixty-nine years ago, we may look forward with confidence to the future. Every advance in science provides a fresh platform from which a new start can be made. The human intellect is still in process of evolution. The power of application and of concentration of thought for the elucidation of scientific problems is by no means exhausted. In science is no hereditary aristocracy. The army of workers is recruited from all classes. The natural ambition of even the private in the ranks to maintain and increase the reputation of the branch of knowledge which he cultivates affords an ample guarantee that the march of science is ever onwards, and justifies us in proclaiming for the next century, as in the one fast ebbing to a close, that Great is Science, and it will prevail.

British Association for the Advancement of Science.

GLASGOW, 1901.

ADDRESS

BY

PROFESSOR ARTHUR W. RÜCKER, M.A., LL.D.,
D.Sc., SEC.R.S.
PRESIDENT.

THE first thought in the minds of all of us to-night is that since we met last year the great Queen, in whose reign nearly all the meetings of the British Association have been held, has passed to her rest.

To Sovereigns most honours and dignities come as of right ; but for some of them is reserved the supreme honour of an old age softened by the love and benedictions of millions ; of a path to the grave, not only magnificent, but watered by the tears both of their nearest and dearest, and of those who, at the most, have only seen them from afar.

This honour Queen Victoria won. All the world knows by what great abilities, by what patient labour, by what infinite tact and kindness, the late Queen gained both the respect of the rulers of nations and the affection of her own subjects.

Her reign, glorious in many respects, was remarkable, outside these islands, for the growth of the Empire ; within and without them, for the drawing nearer of the Crown and the people in mutual trust ; while, during her lifetime, the developments of science and of scientific industry have altered the habits and the thoughts of the whole civilised world.

The representatives of science have already expressed in more formal ways their sorrow at the death of Queen Victoria, and the loyalty and confident hope for the future with which they welcome the accession of King Edward. But none the less, I feel sure that at this, the first meeting of the British Association held in his reign, I am only expressing the universal opinion of all our members when I say that no group of the King's subjects trusts more implicitly than we do in the ability, skill, and judgment which His Majesty has already shown in the exercise of the powers and duties of his august office ; that none sympathise more deeply with the sorrows which two great nations have shared with their Sovereigns ; and that none cry with more fervour, ' Long live the King ! '

But this Meeting of the British Association is not only remarkable as being the first in a new reign. It is also the first in a new century. It is held in Glasgow at a time when your International Exhibition has in a special sense attracted the attention of the world to your city, and when the recent celebration of the ninth jubilee of your University has shown how deeply the prosperity of the present is rooted in the past. What wonder, then, if I take the Chair to which you have called me with some misgivings? Born and bred in the South, I am to preside over a Meeting held in the largest city of Scotland. As your chosen mouth-piece I am to speak to you of science when we stand at the parting of the centuries, and when the achievements of the past and present, and the promise of the future, demand an interpreter with gifts of knowledge and divination to which I cannot pretend. Lastly, I am President of the British Association as a disciple in the home of the master, as a physicist in a city which a physicist has made for ever famous. Whatever the future may have in store for Glasgow, whether your enterprise is still to add wharf to wharf, factory to factory, and street to street, or whether some unforeseen 'tide in the affairs of men' is to sweep energy and success elsewhere, fifty-three years in the history of your city will never be forgotten while civilisation lasts.

More than half a century ago, a mere lad was the first to compel the British Association to listen to the teaching of Joule, and to accept the law of the conservation of energy. Now, alike in the most difficult mathematics and in the conception of the most ingenious apparatus, in the daring of his speculations and in the soundness of his engineering, William Thomson, Lord Kelvin, is regarded as a leader by the science and industry of the whole world.

It is the less necessary to dwell at length upon all that he has done, for Lord Kelvin has not been without honour in his own country. Many of us, who meet here to-night, met last in Glasgow when the University and City had invited representatives of all nations to celebrate the Jubilee of his professorship. For those two or three days learning was surrounded with a pomp seldom to be seen outside a palace. The strange middle-age costumes of all the chief Universities of the world were jostling here, the outward signs that those who were themselves distinguished in the study of Nature had gathered to do honour to one of the most distinguished of them all.

Lord Kelvin's achievements were then described in addresses in every tongue, and therefore I will only remind you that we, assembled here to-night, owe him a heavy debt of gratitude; for the fact that the British Association enters on the twentieth century conscious of a work to do and of the vigour to do it is largely due to his constant presence at its Meetings and to the support he has so ungrudgingly given. We have learned to know not only the work of our great leader, but the man himself and I count myself happy because in his life-long home, under the walls of the University he served so well, and at a Meeting of the

Association which his genius has so often illuminated, I am allowed, as your President, to assure him in your name of the admiration, respect, nay, of the affection, in which we all hold him.

I have already mentioned a number of circumstances which make our Meeting this year noteworthy ; to these I must add that for the first time we have a Section for Education, and the importance of this new departure, due largely to the energy of Professor Armstrong, is emphasised by the fact that the Chair of that Section will be occupied by the Vice-President of the Committee of Council on Education—Sir John Gorst. I will not attempt to forecast the proceedings of the new Section. Education is passing through a transitional stage. The recent debates in Parliament ; the great gifts of Mr. Carnegie ; the discussion as to University organisation in the North of England ; the reconstitution of the University of London ; the increasing importance attached to the application of knowledge both to the investigation of Nature and to the purposes of industry, are all evidence of the growing conviction that without advance in education we cannot retain our position among the nations of the world. If the British Association can provide a platform on which these matters may be discussed in a scientific but practical spirit, free from the misrepresentations of the hustings and the exaggerations of the partisan, it will contribute in no slight measure to the national welfare.

But amid the old and new activities of our meeting the undertone of sadness, which is never absent from such gatherings, will be painfully apparent to many of us at Glasgow. The life-work of Professor Tait has ended amid the gloom of the war-cloud. A bullet, fired thousands of miles away, struck him to the heart, so that in their deaths the father and the brave son, whom he loved so well, were not long divided. Within the last year, too, America has lost Rowland ; Viriamu Jones, who did yeoman's service for education and for science, has succumbed to a long and painful illness ; and one who last year at Bradford seconded the proposal that I should be your President at Glasgow, and who would unquestionably have occupied this Chair before long had he been spared to do so, has unexpectedly been called away. A few months ago we had no reason to doubt that George Francis FitzGerald had many years of health and work before him. He had gained in a remarkable way not only the admiration of the scientific world, but the affection of his friends, and we shall miss sadly one whom we all cared for, and who, we hoped, might yet add largely to the achievements which had made him famous.

The Science of the Nineteenth Century.

Turning from these sad thoughts to the retrospect of the century which has so lately ended, I have found it to be impossible to free myself from the influence of the moment and to avoid, even if it were desirable to avoid, the inclination to look backward from the standpoint of to-day.

Two years ago Sir Michael Foster dealt with the work of the century

as a whole. Last year Sir William Turner discussed in greater detail the growth of a single branch of science. A third and humbler task remains, viz., to fix our attention on some of the hypotheses and assumptions on which the fabric of modern theoretical science has been built, and to inquire whether the foundations have been so 'well and truly' laid that they may be trusted to sustain the mighty superstructure which is being raised upon them.

The moment is opportune. The three chief conceptions which for many years have dominated physical as distinct from biological science have been the theories of the existence of atoms, of the mechanical nature of heat, and of the existence of the ether.

Dalton's atomic theory was first given to the world by a Glasgow professor—Thomas Thomson—in the year 1807, Dalton having communicated it to him in 1804. Rumford's and Davy's experiments on the nature of heat were published in 1798 and 1799 respectively; and the celebrated Bakerian Lecture, in which Thomas Young established the undulatory theory by explaining the interference of light, appeared in the 'Philosophical Transactions' in 1801. The keynotes of the physical science of the nineteenth century were thus struck, as the century began, by four of our fellow-countrymen, one of whom—Sir Benjamin Thompson, Count Rumford—preferred exile from the land of his birth to the loss of his birthright as a British citizen.

Doubts as to Scientific Theories.

It is well known that of late doubts have arisen as to whether the atomic theory, with which the mechanical theory of heat is closely bound up, and the theory of the existence of an ether have not served their purpose, and whether the time has not come to reconsider them.

The facts that Professor Poincaré, addressing a congress of physicists in Paris, and Professor Poynting, addressing the Physical Section of the Association, have recently discussed the true meaning of our scientific methods of interpretation; that Dr. James Ward has lately delivered an attack of great power on many positions which eminent scientific men have occupied; and that the approaching end of the nineteenth century led Professor Hæckel to define in a more popular manner his own very definite views as to the solution of the 'Riddle of the Universe,' are perhaps a sufficient justification of an attempt to lay before you the difficulties which surround some of these questions.

To keep the discussion within reasonable limits I shall illustrate the principles under review by means of the atomic theory, with comparatively little reference to the ether, and we may also at first confine our attention to inanimate objects.

The Construction of a Model of Nature.

A natural philosopher, to use the old phrase, even if only possessed of the most superficial knowledge, would attempt to bring some order into the results of his observation of Nature by grouping together statements with regard to phenomena which are obviously related. The aim of modern science goes far beyond this. It not only shows that many phenomena are related which at first sight have little or nothing in common, but, in so doing, also attempts to explain the relationship.

Without spending time on a discussion of the meaning of the word 'explanation,' it is sufficient to say that our efforts to establish relationships between phenomena often take the form of attempting to prove that, if a limited number of assumptions are granted as to the constitution of matter, or as to the existence of quasi-material entities, such as caloric, electricity, and the ether, a wide range of observed facts falls into order as a necessary consequence of the assumptions. The question at issue is whether the hypotheses which are at the base of the scientific theories now most generally accepted are to be regarded as accurate descriptions of the constitution of the universe around us, or merely as convenient fictions.

Convenient fictions be it observed, for even if they are fictions they are not useless. From the practical point of view it is a matter of secondary importance whether our theories and assumptions are correct, if only they guide us to results which are in accord with facts. The whole fabric of scientific theory may be regarded merely as a gigantic 'aid to memory'; as a means for producing apparent order out of disorder by codifying the observed facts and laws in accordance with an artificial system, and thus arranging our knowledge under a comparatively small number of heads. The simplification introduced by a scheme which, however imperfect it may be, enables us to argue from a few first principles, makes theories of practical use. By means of them we can foresee the results of combinations of causes which would otherwise elude us. We can predict future events, and can even attempt to argue back from the present to the unknown past.

But it is possible that these advantages might be attained by means of axioms, assumptions, and theories based on very false ideas. A person who thought that a river was really a streak of blue paint might learn as much about its direction from a map as one who knew it as it is. It is thus conceivable that we might be able, not indeed to construct, but to imagine, something more than a mere map or diagram, something which might even be called a working model of inanimate objects, which was nevertheless very unlike the realities of nature. Of course, the agreement between the action of the model and the behaviour of the things it was designed to represent would probably be imperfect, unless the one were a facsimile of the other; but it is conceivable that the correlation of natural phenomena could be imitated,

with a large measure of success, by means of an imaginary machine, which shared with a map or diagram the characteristic that it was in many ways unlike the things it represented, but might be compared to a model in that the behaviour of the things represented could be predicted from that of the corresponding parts of the machine.

We might even go a step further. If the laws of the working of the model could be expressed by abstractions, as, for example, by mathematical formulæ, then, when the formulæ were obtained, the model might be discarded, as probably unlike that which it was made to imitate, as a mere aid in the construction of equations, to be thrown aside when the perfect structure of mathematical symbols was erected.

If this course were adopted we should have given up the attempt to know more of the nature of the objects which surround us than can be gained by direct observation, but might nevertheless have learned how these objects would behave under given circumstances.

We should have abandoned the hope of a physical explanation of the properties of inanimate Nature, but should have secured a mathematical description of her operations.

There is no doubt that this is the easiest path to follow. Criticism is avoided if we admit from the first that we cannot go below the surface ; cannot know anything about the constitution of material bodies ; but must be content with formulating a description of their behaviour by means of laws of Nature expressed by equations.

But if this is to be the end of the study of Nature, it is evident that the construction of the model is not an essential part of the process. The model is used merely as an aid to thinking ; and if the relations of phenomena can be investigated without it, so much the better. The highest form of theory—it may be said—the widest kind of generalisation, is that which has given up the attempt to form clear mental pictures of the constitution of matter, which expresses the facts and the laws by language and symbols which lead to results that are true, whatever be our view as to the real nature of the objects with which we deal. From this point of view the atomic theory becomes not so much false as unnecessary ; it may be regarded as an attempt to give an unnatural precision to ideas which are and must be vague.

Thus, when Rumford found that the mere friction of metals produced heat in unlimited quantity, and argued that heat was therefore a mode of motion, he formed a clear mental picture of what he believed to be occurring. But his experiments may be quoted as proving only that energy can be supplied to a body in indefinite quantity, and when supplied by doing work against friction it appears in the form of heat.

By using this phraseology we exchange a vivid conception of moving atoms for a colourless statement as to heat energy, the real nature of which we do not attempt to define ; and methods which thus evade the problem of the nature of the things which the symbols in our equations represent have been prosecuted with striking success, at all events

within the range of a limited class of phenomena. A great school of chemists, building upon the thermodynamics of Willard Gibbs and the intuition of Van t'Hoff, have shown with wonderful skill that, if a sufficient number of the data of experiment are assumed, it is possible, by the aid of thermodynamics, to trace the form of the relations between many physical and chemical phenomena without the help of the atomic theory.

But this method deals only with matter as our coarse senses know it ; it does not pretend to penetrate beneath the surface.

It is therefore with the greatest respect for its authors, and with a full recognition of the enormous power of the weapons employed, that I venture to assert that the exposition of such a system of tactics cannot be regarded as the last word of science in the struggle for the truth.

Whether we grapple with them, or whether we shirk them ; however much or however little we can accomplish without answering them, the questions still force themselves upon us : Is matter what it seems to be ? Is interplanetary space full or empty ? Can we argue back from the direct impressions of our senses to things which we cannot directly perceive ; from the phenomena displayed by matter to the constitution of matter itself ?

It is these questions which we are discussing to-night, and we may therefore, as far as the present address is concerned, put aside, once for all, methods of scientific exposition in which an attempt to form a mental picture of the constitution of matter is practically abandoned, and devote ourselves to the inquiries whether the effort to form such a picture is legitimate, and whether we have any reason to believe that the sketch which science has already drawn is to some extent a copy, and not a mere diagram, of the truth.

Successive Steps in the Analysis of Matter.

In dealing, then, with the question of the constitution of matter and the possibility of representing it accurately, we may grant at once that the ultimate nature of things is, and must remain, unknown ; but it does not follow that immediately below the complexities of the superficial phenomena which affect our senses there may not be a simpler machinery of the existence of which we can obtain evidence, indirect indeed but conclusive.

The fact that the apparent unity which we call the atmosphere can be resolved into a number of different gases is admitted ; though the ultimate nature of oxygen, nitrogen, argon, carbonic acid, and water vapour is as unintelligible as that of air as a whole, so that the analysis of air may be said to have substituted many incomprehensibles for one.

Nobody, however, looks at the question from this point of view. It is recognised that an investigation into the proximate constitution of things may be useful and successful, even if their ultimate nature is beyond our ken.

Nor need the analysis stop at the first step. Water vapour and carbonic acid, themselves constituents of the atmosphere, are in turn resolved into their elements hydrogen, oxygen, and carbon, which, without a formal discussion of the criteria of reality, we may safely say are as real as air itself.

Now, at what point must this analysis stop if we are to avoid crossing the boundary between fact and fiction? Is there any fundamental difference between resolving air into a mixture of gases and resolving an elementary gas into a mixture of atoms and ether?

There are those who cry halt! at the point at which we divide a gas into molecules, and their first objection seems to be that molecules and atoms cannot be directly perceived, cannot be seen or handled, and are mere conceptions, which have their uses, but cannot be regarded as realities.

It is easiest to reply to this objection by an illustration.

The rings of Saturn appear to be continuous masses separated by circular rifts. This is the phenomenon which is observed through a telescope. By no known means can we ever approach or handle the rings; yet everybody who understands the evidence now believes that they are not what they appear to be, but consist of minute moonlets, closely packed indeed, but separate the one from the other.

In the first place Maxwell proved mathematically that if a Saturnian ring were a continuous solid or fluid mass it would be unstable and would necessarily break into fragments. In the next place, if it were possible for the ring to revolve like a solid body, the inmost parts would move slowest, while a satellite moves faster the nearer it is to a planet. Now spectroscopic observation, based on the beautiful method of Sir W. Huggins, shows not only that the inner portions of the ring move the more rapidly, but that the actual velocities of the outer and inner edges are in close accord with the theoretical velocities of satellites at like distances from the planet.

This and a hundred similar cases prove that it is possible to obtain convincing evidence of the constitution of bodies between whose separate parts we cannot directly distinguish, and I take it that a physicist who believes in the reality of atoms thinks that he has as good reason for dividing an apparently continuous gas into molecules as he has for dividing the apparently continuous Saturnian rings into satellites. If he is wrong it is not the fact that molecules and satellites alike cannot be handled and cannot be seen as individuals, that constitutes the difference between the two cases.

It may, however, be urged that atoms and the ether are alleged to have properties different from those of matter in bulk, of which alone our senses take direct cognisance, and that therefore it is impossible to prove their existence by evidence of the same cogency as that which may prove the existence of a newly discovered variety of matter or of a portion of matter too small or too distant to be seen.

This point is so important that it requires full discussion, but in dealing with it, it is necessary to distinguish carefully between the validity of the arguments which support the earlier and more fundamental propositions of the theory ; and the evidence brought forward to justify mere speculative applications of its doctrines which might be abandoned without discarding the theory itself. The proof of the theory must be carried out step by step.

The first step is concerned wholly with some of the most general properties of matter, and consists in the proof that those properties are either absolutely unintelligible, or that, in the case of matter of all kinds, we are subject to an illusion similar to that the results of which we admit in the case of Saturn's rings, clouds, smoke, and a number of similar instances. The believer in the atomic theory asserts that matter exists in a particular state ; that it consists of parts which are separate and distinct the one from the other, and as such are capable of independent movements.

Up to this point no question arises as to whether the separate parts are, like grains of sand, mere fragments of matter ; or whether, though they are the bricks of which matter is built, they have, as individuals, properties different from those of masses of matter large enough to be directly perceived. If they are mere fragments of ordinary matter, they cannot be used as aids in explaining those qualities of matter which they themselves share.

We cannot explain things by the things themselves. If it be true that the properties of matter are the product of an underlying machinery, that machinery cannot itself have the properties which it produces, and must, to that extent at all events, differ from matter in bulk as it is directly presented to the senses.

If, however, we can succeed in showing that if the separate parts have a limited number of properties (different, it may be, from those of matter in bulk), the many and complicated properties of matter can, to a considerable extent, be explained as consequences of the constitution of these separate parts ; we shall have succeeded in establishing, with regard to quantitative properties, a simplification similar to that which the chemist has established with regard to varieties of matter. The many will have been reduced to the few.

The proofs of the physical reality of the entities discovered by means of the two analyses must necessarily be different. The chemist can actually produce the elementary constituents into which he has resolved a compound mass. No physicist or chemist can produce a single atom separated from all its fellows, and show that it possesses the elementary qualities he assigns to it. The cogency of the evidence for any suggested constitution of atoms must vary with the number of facts which the hypothesis that they possess that constitution explains.

Let us take, then, two steps in their proper order, and inquire, first,

whether there is valid ground for believing that all matter is made up of discrete parts ; and secondly, whether we can have any knowledge of the constitution or properties which those parts possess.

The Coarse-grainedness of Matter.

Matter in bulk appears to be continuous. Such substances as water or air appear to the ordinary observer to be perfectly uniform in all their properties and qualities, in all their parts.

The hasty conclusion that these bodies are really uniform is, nevertheless, unthinkable.

In the first place the phenomena of diffusion afford conclusive proof that matter when apparently quiescent is in fact in a state of internal commotion. I need not recapitulate the familiar evidence to prove that gases and many liquids when placed in communication interpenetrate or diffuse into each other ; or that air, in contact with a surface of water, gradually becomes laden with water vapour, while the atmospheric gases in turn mingle with the water. Such phenomena are not exhibited by liquids and gases alone, nor by solids at high temperatures only. Sir W. Roberts Austen has placed pieces of gold and lead in contact at a temperature of 18° C. After four years the gold had travelled into the lead to such an extent that not only were the two metals united, but, on analysis, appreciable quantities of the gold were detected even at a distance of more than 5 millimetres from the common surface, while within a distance of three-quarters of a millimetre from the surface gold had penetrated into the lead to the extent of 1 oz. 6 dwts. per ton, an amount which could have been profitably extracted.

Whether it is or is not possible to devise any other intelligible account of the cause of such phenomena, it is certain that a simple and adequate explanation is found in the hypothesis that matter consists of discrete parts in a state of motion, which can penetrate into the spaces between the corresponding parts of surrounding bodies.

The hypothesis thus framed is also the only one which affords a rational explanation of other simple and well known facts. If matter is regarded as a continuous medium the phenomena of expansion are unintelligible. There is, apparently, no limit to the expansion of matter, or, to fix our attention on one kind of matter, let us say to the expansion of a gas ; but it is inconceivable that a continuous material which fills or is present in every part of a given space could also be present in every part of a space a million times as great. Such a statement might be made of a mathematical abstraction ; it cannot be true of any real substance or thing. If, however, matter consists of discrete particles, separated from each other either by empty space or by something different from themselves, we can at once understand that expansion and contraction may be nothing more than the mutual separation or approach of these particles.

Again, no clear mental picture can be formed of the phenomena of

heat unless we suppose that heat is a mode of motion. In the words of Rumford, it is 'extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated in the manner the heat was excited and communicated in [his] experiment [on friction] except it be motion.'¹ And if heat be motion there can be no doubt that it is the fundamental particles of matter which are moving. For the motion is not visible, is not motion of the body as a whole, while diffusion, which is a movement of matter, goes on more quickly as the temperature rises, thereby proving that the internal motions have become more rapid, which is exactly the result which would follow if these were the movements which constitute sensible heat.

Combining, then, the phenomena of diffusion, expansion, and heat, it is not too much to say that no hypotheses which make them intelligible have ever been framed other than those which are at the basis of the atomic theory.

Many other considerations also point to the same conclusion. Many years ago Lord Kelvin gave independent arguments, based on the properties of gases, on the constitution of the surfaces of liquids, and on the electric properties of metals, all of which indicate that matter is, to use his own phrase, coarse-grained—that it is not identical in constitution throughout, but that adjacent minute parts are distinguishable from each other by being either of different natures or in different states.

And here it is necessary to insist that all these fundamental proofs are independent of the nature of the particles or granules into which matter must be divided.

The particles, for instance, need not be different in kind from the medium which surrounds and separates them. It would suffice if they were what may be called singular parts of the medium itself, differing from the rest only in some peculiar state of internal motion or of distortion, or by being in some other way earmarked as distinct individuals. The view that the constitution of matter is atomic may and does receive support from theories in which definite assumptions are made as to the constitution of the atoms; but when, as is often the case, these assumptions introduce new and more recondite difficulties, it must be remembered that the fundamental hypothesis—that matter consists of discrete parts, capable of independent motions—is forced upon us by facts and arguments which are altogether independent of what the nature and properties of these separate parts may be.

As a matter of history the two theories, which are not by any means mutually exclusive, that atoms are particles which can be treated as distinct in kind from the medium which surrounds them, and that they are parts of that medium existing in a special state, have both played a large part in the theoretical development of the atomic hypothesis. The atoms of Waterston, Clausius, and Maxwell were particles. The vortex-atoms

¹ *Phil. Trans.*, 1798, p. 99.

of Lord Kelvin, and the strain-atoms (if I may call them so) suggested by Mr. Larmor, are states of a primary medium which constitutes a physical connection between them, and through which their mutual actions arise and are transmitted.

Properties of the Basis of Matter.

It is easy to show that, whichever alternative be adopted, we are dealing with something, whether we consider it under the guise of separate particles or of differentiated portions of the medium, which has properties different from those of matter in bulk.

For if the basis of matter had the same constitution as matter, the irregular heat movements could hardly be maintained either against the viscosity of the medium or the frittering away of energy of motion which would occur during the collisions between the particles. Thus, even in the case in which a hot body is prevented from losing heat to surrounding objects, its sensible heat should spontaneously decay by a process of self-cooling. No such phenomenon is known, and though on this, as on all other points, the limits of our knowledge are fixed by the uncertainty of experiment, we are compelled to admit that, to all appearance, the fundamental medium, if it exists, is unlike a material medium, in that it is non-viscous; and that the particles, if they exist, are so constituted that energy is not frittered away when they collide. In either case, we are dealing with something different from matter itself in the sense that, though it is the basis of matter, it is not identical in all its properties with matter.

The idea therefore that entities exist possessing properties different from those of matter in bulk is not introduced at the end of a long and recondite investigation to explain facts with which none but experts are acquainted. It is forced upon us at the very threshold of our study of Nature. Either the properties of matter in bulk cannot be referred to any simpler structure, or that simpler structure must have properties different from those of matter in bulk as we directly knew it—properties which can only be inferred from the results which they produce.

No *a priori* argument against the possibility of our discovering the existence of quasi-material substances, which are nevertheless different from matter, can prove the negative proposition that such substances cannot exist. It is not a self-evident truth that no substance other than ordinary matter can have an existence as real as that of matter itself. It is not axiomatic that matter cannot be composed of parts whose properties are different from those of the whole. To assert that even if such substances and such parts exist no evidence however cogent could convince us of their existence is to beg the whole question at issue; to decide the cause before it has been heard.

We must therefore adhere to the standpoint adopted by most scientific men, viz., that the question of the existence of ultra-physical entities, such as atoms and the ether, is to be settled by the evidence, and must not be ruled out as inadmissible on *a priori* grounds.

On the other hand, it is impossible to deny that, if the mere entry on the search for the concealed causes of physical phenomena is not a trespass on ground we have no right to explore, it is at all events the beginning of a dangerous journey.

The wraiths of phlogiston, caloric, luminiferous corpuscles, and a crowd of other phantoms haunt the investigator, and as the grim host vanishes into nothingness he cannot but wonder if his own conceptions of atoms and of the ether

‘shall dissolve,
And, like this insubstantial pageant faded,
Leave not a wrack behind.’

But though science, like Bunyan’s hero, has sometimes had to pass through the ‘Valley of Humiliation,’ the spectres which meet it there are not really dangerous if they are boldly faced. The facts that mistakes have been made, that theories have been propounded, and for a time accepted, which later investigations have disproved, do not necessarily discredit the method adopted. In scientific theories, as in the world around us, there is a survival of the fittest, and Dr. James Ward’s unsympathetic account of the blunders of those whose work, after all, has shed glory on the nineteenth century, might *mutatis mutandis* stand for a description of the history of the advance of civilisation. ‘The story of the progress so far,’ he tells us, ‘is briefly this: Divergence between theory and fact one part of the way, the wreckage of abandoned fictions for the rest, with an unattainable goal of phenomenal nihilism and ultra-physical mechanism beyond.’¹

‘The path of progress,’ says Professor Karl Pearson, ‘is strewn with the wreck of nations. Traces are everywhere to be seen of the hecatombs of inferior races, and of victims who found not the narrow way to the greater perfection. Yet these dead peoples are, in very truth, the stepping-stones on which mankind has arisen to the higher intellectual and deeper emotional life of to-day.’²

It is only necessary to add that the progress of society is directed towards an unattainable goal of universal contentment, to make the parallel complete.

And so, in the one case as in the other, we may leave ‘the dead to bury their dead.’ The question before us is not whether we too may not be trusting to false ideas, erroneous experiments, evanescent theories. No doubt we are; but, without making an insolent claim to be better than our fathers, we may fairly contend that, amid much that is uncertain and temporary, some of the fundamental conceptions, some of the root-ideas of science, are so grounded on reason and fact that we cannot but regard them as an aspect of the very truth.

Enough has, perhaps, now been said on this point for my immediate

¹ James Ward, *Naturalism and Agnosticism*, vol. i. p. 153.

² Karl Pearson, *National Life from the Standpoint of Science*, p. 62.

purpose. The argument as to the constitution of matter could be developed further in the manner I have hitherto adopted, viz., by series of propositions, the proof of each of which is based upon a few crucial phenomena. In particular, if matter is divided into moving granules or particles, the phenomenon of cohesion proves that there must be mutual actions between them analogous to those which take place between large masses of matter, and which we ascribe to force, thereby indicating the regular, unvarying operation of active machinery which we have not yet the means of adequately understanding. For the moment, I do not wish to extend the line of reasoning that has been followed. My main object is to show that the notion of the existence of ultra-physical entities and the leading outlines of the atomic theory are forced upon us at the beginning of our study of Nature, not only by *a priori* considerations, but in the attempt to comprehend the results of even the simplest observation. These outlines cannot be effaced by the difficulties which undoubtedly arise in filling up the picture. The cogency of the proof that matter is coarse-grained is in no way affected by the fact that we may have grave doubts as to the nature of the granules. Nay, it is of the first importance to recognise that, though the fundamental assumptions of the atomic theory receive overwhelming support from a number of more detailed arguments, they are themselves almost of the nature of axioms, in that the simplest phenomena are unintelligible if they are abandoned.

The Range of the Atomic Theory.

It would be most unfair, however, to the atomic theory to represent it as depending on one line of reasoning only, or to treat its evidence as bounded by the very general propositions I have discussed.

It is true that as the range of the theory is extended the fundamental conception that matter is granular must be expanded and filled in by supplementary hypotheses as to the constitution of the granules. It may also be admitted that no complete or wholly satisfactory description of that constitution can as yet be given; that perfection has not yet been attained here or in any other branch of science; but the number of facts which can be accounted for by the theory is very large compared with the number of additional hypotheses which are introduced; and the cumulative weight of the additional evidence obtained by the study of details is such as to add greatly to the strength of the conviction that, in its leading outlines, the theory is true.

It was originally suggested by the facts of chemistry, and though, as we have seen, a school of chemists now thrusts it into the background, it is none the less true, in the words of Dr. Thorpe, that 'every great advance in chemical knowledge during the last ninety years finds its interpretation in [Dalton's] theory.'¹

The principal mechanical and thermal properties of gases have been

¹ Thorpe, *Essays on Historical Chemistry*, 1894, p. 368.

explained, and in large part discovered, by the aid of the atomic theory ; and, though there are outstanding difficulties, they are, for the most part, related to the nature of the atoms and molecules, and do not affect the question as to whether they exist.

The fact that different kinds of light all travel at the same speed in interplanetary space, while they move at different rates in matter, is explained if matter is coarse-grained. But to attempt to sum up all this evidence would be to recite a text-book on physics. It must suffice to say that it is enormous in extent and varied in character, and that the atomic theory imparts a unity to all the physical sciences which has been attained in no other way.

I must, however, give a couple of instances of the wonderful success which has been achieved in the explanation of physical phenomena by the theory we are considering, and I select them because they are in harmony with the line of argument I have been pursuing.

When a piece of iron is magnetised its behaviour is different according as the magnetic force applied to it is weak, moderate, or strong. When a certain limit is passed the iron behaves as a non-magnetic substance to all further additions of magnetic force. With strong forces it does and with very weak forces it does not remain magnetised when the force ceases to act. Professor Ewing has imitated all the minute details of these complicated properties by an arrangement of small isolated compass needles to represent the molecules. It may fairly be said that as far as this particular set of phenomena is concerned a most instructive working model based on the molecular theory has not only been imagined but constructed.

The next illustration is no less striking. We may liken a crowd of molecules to a fog ; but while the fog is admitted by everybody to be made up of separate globules of water, the critics of scientific method are sometimes apt to regard the molecules as mere fictions of the imagination. If, however, we could throw the molecules of a highly rarefied gas into such a state that vapour condensed on them, so that each became the centre of a water-drop, till the host of invisible molecules was, as it were, magnified by accretion into a visible mist, surely no stronger proof of their reality could be desired. Yet there is every reason to believe that something very like this has been accomplished by Mr. C. T. R. Wilson and Professor J. J. Thomson.

It is known that it is comparatively difficult to produce a fog in damp air if the mixture consists of air and water-vapour alone. The presence of particles of very fine dust facilitates the process. It is evident that the vapour condenses on the dust particles and that a nucleus of some kind is necessary on which each drop may form. But electrified particles also act as nuclei ; for if a highly charged body from which electricity is escaping be placed near a steam jet, the steam condenses ; and a cloud is also formed in dust-free air more easily than would otherwise be the case if electricity is discharged into it.

Again, according to accepted theory, when a current of electricity flows through a gas some of the atoms are divided into parts which carry positive and negative charges as they move in opposite directions, and unless this breaking-up occurs a gas does not conduct electricity. But a gas can be made a conductor merely by allowing the Röntgen rays or the radiation given off by uranium to fall upon it. A careful study of the facts shows that it is probable that some of the atoms have been broken up by the radiation, and that their oppositely electrified parts are scattered among their unaltered fellows. Such a gas is said to be ionised.

Thus by these two distinct lines of argument we come to the conclusions:—1st, that the presence of electrified particles promotes the formation of mist, and 2nd, that in an ionised gas such electrified particles are provided by the breaking-up of atoms.

The two conclusions will mutually support each other if it can be shown that a mist is easily formed in ionised air. This was tested by Mr. Wilson, who showed that in such air mist is formed as though nuclei were present, and thus in the cloud we have visible evidence of the presence of the divided atoms. If then we cannot handle the individual molecules we have at least some reason to believe that a method is known of seizing individuals, or parts of individuals, which are in a special state, and of wrapping other matter round them till each one is the centre of a discrete particle of a visible fog.

I have purposely chosen this illustration, because the explanation is based on a theory—that of ionisation—which is at present subjected to hostile criticism. It assumes that an electrical current is nothing more than the movement of charges of electricity. But magnets placed near to an electric current tend to set themselves at right angles to its direction; a fact on which the construction of telegraphic instruments is based. Hence if the theory be true, a similar effect ought to be produced by a moving charge of electricity. This experiment was tried many years ago in the laboratory of Helmholtz by Rowland, who caused a charged disc to spin rapidly near a magnet. The result was in accord with the theory; the magnet moved as though acted upon by an electric current. Of late, however, M. Crémieu has investigated the matter afresh, and has obtained results which, according to his interpretation, were inconsistent with that of Rowland.

M. Crémieu's results are already the subject of controversy,¹ and are, I believe, likely to be discussed in the Section of Physics. This is not the occasion to enter upon a critical discussion of the question at issue, and I refer to it only to point out that though, if M. Crémieu's result were upheld, our views as to electricity would have to be modified, the foundations of the atomic theory would not be shaken.

¹ See *Phil. Mag.*, July 1901, p. 144; and *Johns Hopkins University Circulars*, xx.-No. 152, May-June 1901, p. 78.

It is, however, from the theory of ions that the most far-reaching speculations of science have recently received unexpected support. The dream that matter of all kinds will some day be proved to be fundamentally the same has survived many shocks. The opinion is consistent with the great generalisation that the properties of elements are a periodic function of their atomic weights. Sir Norman Lockyer has long been a prominent exponent of the view that the spectra of the stars indicate the reduction of our so-called elements to simpler forms, and now Professor J. J. Thomson believes that we can break off from an atom a part, the mass of which is not more than one thousandth of the whole, and that these corpuscles, as he has named them, are the carriers of the negative charge in an electric current. If atoms are thus complex, not only is the *a priori* probability increased that the different structures which we call elements may all be built of similar bricks, but the discovery by Lenard that the ease with which the corpuscles penetrate different bodies depends only on the density of the obstacles, and not on their chemical constitution, is held by Professor Thomson to be 'a strong confirmation of the view that the atoms of the elementary substances are made up of simpler parts, all of which are alike.'¹ On the present occasion, however, we are occupied rather with the foundations than with these ultimate ramifications of the atomic theory; and having shown how wide its range is, I must, to a certain extent, retrace my steps and return to the main line of my argument.

The Properties of Atoms and Molecules.

For if it be granted that the evidence that matter is coarse-grained and is formed of separate atoms and molecules is too strong to be resisted, it may still be contended that we can know little or nothing of the sizes and properties of the molecules.

It must be admitted that though the fundamental postulates are always the same, different aspects of the theory, which have not in all cases been successfully combined, have to be developed when it is applied to different problems; but in spite of this there is little doubt that we have some fairly accurate knowledge of molecular motions and magnitudes.

If a liquid is stretched into a very thin film, such as a soap bubble, we should expect indications of a change in its properties when the thickness of the film is not a very large multiple of the average distance between two neighbouring molecules. In 1890 Sohncke² detected evidence of such a change in films of the average thickness of 106 millionths

¹ For the most recent account of this subject see an article on 'Bodies smaller than Atoms,' by Professor J. J. Thomson in the *Popular Science Monthly* (The Science Press), August 1901.

² *Wied. Ann.*, 1890, xl. pp. 345-355.

of a millimetre ($\mu\mu$), and quite recently Rudolph Weber found it in an oil-film when the thickness was $115 \mu\mu$.¹

Taking the mean of these numbers and combining the results of different variants of the theory we may conclude that a film should become unstable and tend to rupture spontaneously somewhere between the thicknesses of 110 and $55 \mu\mu$, and Professor Reinold and I found by experiment that this instability is actually exhibited between the thicknesses of 96 and $45 \mu\mu$.² There can therefore be little doubt that the first approach to molecular magnitudes is signalled when the thickness of a film is somewhat less than $100 \mu\mu$, or 4 millionths of an inch.

Thirteen years ago I had the honour of laying before the Chemical Society a résumé of what was then known on these subjects,³ and I must refer to that lecture or to the most recent edition of O. E. Meyer's work on the kinetic theory of gases⁴ for the evidence that various independent lines of argument enable us to estimate quantities very much less than 4 millionths of an inch, which is perhaps from 500 to 1,000 times greater than the magnitude which, in the present state of our knowledge, we can best describe as the diameter of a molecule.

Confining our attention, however, to the larger quantities, I will give one example to show how strong is the cumulative force of the evidence as to our knowledge of the magnitudes of molecular quantities.

We have every reason to believe that though the molecules in a gas frequently collide with each other, yet in the case of the more perfect gases the time occupied in collisions is small compared with that in which each molecule travels undisturbed by its fellows. The average distance travelled between two successive encounters is called the mean free path, and, for the reason just given, the question of the magnitude of this distance can be attacked without any precise knowledge of what a molecule is, or of what happens during an encounter.

Thus the mean free path can be determined, by the aid of the theory, either from the viscosity of the gas or from the thermal conductivity. Using figures given in the latest work on the subject,⁵ and dealing with one gas only, as a fair sample of the rest, the lengths of the mean free path of hydrogen as determined by these two independent methods differ only by about 3 per cent. Further, the mean of the values which I gave in the lecture already referred to differed only by about 6 per cent. from the best modern result, so that no great change has been introduced during the last thirteen years.

It may, however, be argued that these concordant values are all obtained by means of the same theory, and that a common error may affect them all. In particular, some critics have of late been inclined to

¹ *Annalen der Physik*, 1901, iv. pp. 706–721.

² *Phil. Trans.*, 1893, 184, pp. 505–529.

³ *Chem. Soc. Trans.*, liii., March 1888, pp. 222–262.

⁴ *Kinetic Theory of Gases*, O. E. Meyer, 1899. Translated by R. E. Baynes.

⁵ Meyer's *Kinetic Theory of Gases* (see above).

discredit the atomic theory by pointing out that the strong statements which have sometimes been made as to the equality, among themselves, of atoms or molecules of the same kind may not be justified, as the equality may be that of averages only, and be consistent with a considerable variation in the sizes of individuals.

Allowing this argument more weight than it perhaps deserves, it is easy to show that it cannot affect seriously our knowledge of the length of the mean free path.

Professor George Darwin¹ has handled the problem of a mixture of unequal spherical bodies in the particular case in which the sizes are distributed according to the law of errors, which would involve far greater inequalities than can occur among atoms. Without discussing the precise details of his problem it is sufficient to say that in the case considered by him the length of the mean free path is $\frac{1}{2}$ of what it would be if the particles were equal. Hence were the inequalities of atoms as great as in this extreme case, the reduction of the mean free path in hydrogen could only be from 185 to 119 $\mu\mu$; but they must be far less, and therefore the error, if any, due to this cause could not approach this amount. It is probably inappreciable.

Such examples might be multiplied, but the one I have selected is perhaps sufficient to illustrate my point, viz., that considerable and fairly accurate knowledge can be obtained as to molecular quantities by the aid of theories the details of which are provisional, and are admittedly capable of improvement.

Is the Model Unique?

But the argument that a correct result may sometimes be obtained by reasoning on imperfect hypotheses raises the question as to whether another danger may not be imminent. To be satisfactory our model of Nature must be unique, and it must be impossible to imagine any other which agrees equally well with the facts of experiment. If a large number of hypotheses could be framed with equal claims to validity, that fact would alone raise grave doubts as to whether it were possible to distinguish between the true and the false. Thus Professor Poincaré has shown that an infinite number of dynamical explanations can be found for any phenomenon which satisfies certain conditions. But though this consideration warns us against the too ready acceptance of explanations of isolated phenomena, it has no weight against a theory which embraces so vast a number of facts as those included by the atomic theory. It does not follow that, because a number of solutions are all formally dynamical, they are therefore all equally admissible. The pressure of a gas may be explained as the result of a shower of blows delivered by molecules, or by a repulsion between the various parts of a continuous medium. Both solutions are expressed in dynamical language; but one is, and the other

¹ *Phil. Trans.*, 180.

is not, compatible with the observed phenomena of expansion. The atomic theory must hold the field until another can be found which is not inferior as an explanation of the fundamental difficulties as to the constitution of matter, and is, at the same time, not less comprehensive.

On the whole, then, the question as to whether we are attempting to solve a problem which has an infinite number of solutions may be put aside until one solution has been found which is satisfactory in all its details. We are in a sufficient difficulty about that to make the rivalry of a second of the same type very improbable.

The Phenomena of Life.

But it may be asked—nay, it has been asked—may not the type of our theories be radically changed? If this question does not merely imply a certain distrust in our own powers of reasoning, it should be supported by some indication of the kind of change which is conceivable.

Perhaps the chief objection which can be brought against physical theories is that they deal only with the inanimate side of Nature, and largely ignore the phenomena of life. It is therefore in this direction, if in any, that a change of type may be expected. I do not propose to enter at length upon so difficult a question, but, however we may explain or explain away the characteristics of life, the argument for the truth of the atomic theory would only be affected if it could be shown that living matter does not possess the thermal and mechanical properties, to explain which the atomic theory has been framed. This is so notoriously not the case that there is the gravest doubt whether life can in any way interfere with the action within the organism of the laws of matter in bulk belonging to the domain of mechanics, physics, and chemistry.

Probably the most cautious opinion that could now be expressed on this question is that, in spite of some outstanding difficulties which have recently given rise to what is called Neovitalism, there is no conclusive evidence that living matter can suspend or modify any of the natural laws which would affect it if it were to cease to live. It is possible that though subject to these laws the organism while living may be able to employ, or even to direct, their action within itself for its own benefit, just as it unquestionably does make use of the processes of external nature for its own purposes; but if this be so, the seat of the controlling influence is so withdrawn from view that on the one hand its very existence may be denied, while, on the other hand, Professor Hæckel, following Vogt, has recently asserted that ‘matter and ether are not dead, and only moved by extrinsic force; but they are endowed with sensation and will; they experience an inclination for condensation, a dislike for strain; they strive after the one and struggle against the other.’¹

But neither unproved assertions of this kind nor the more refined attempts that have been made by others to bring the phenomena of life

¹ *Riddle of the Universe* (English translation), 1900, p. 380.

and of dead matter under a common formula touch the evidence for the atomic theory. The question as to whether matter consists of elements capable of independent motion is prior to and independent of the further questions as to what these elements are, and whether they are alive or dead.

The physicist, if he keeps to his business, asserts, as the bases of the atomic theory, nothing more than that he who declines to admit that matter consists of separate moving parts must regard many of the simplest phenomena as irreconcilable and unintelligible, in spite of the fact that means of reconciling them are known to everybody, in spite of the fact that the reconciling theory gives a general correlation of an enormous number of phenomena in every branch of science, and that the outstanding difficulties are connected, not so much with the fundamental hypotheses that matter is composed of distinguishable entities which are capable of separate motions as with the much more difficult problem of what these entities are.

On these grounds the physicist may believe that, though he cannot handle or see them, the atoms and molecules are as real as the ice crystals in a cirrus cloud which he cannot reach ; as real as the unseen members of a meteoric swarm whose death-glow is lost in the sunshine, or which sweep past us, unentangled, in the night.

If the confidence that his methods are weapons with which he can fight his way to the truth were taken from the scientific explorer, the paralysis which overcomes those who believe that they are engaged in a hopeless task would fall upon him.

Physiology has specially flourished since physiologists have believed that it is possible to master the physics and chemistry of the framework of living things, and since they have abandoned the attitude of those who placed in the foreground the doctrine of the vital force. To supporters of that doctrine the principle of life was not a hidden directing power which could perhaps whisper an order that the flood-gates of reservoirs of energy should now be opened and now closed, and could, at the most, work only under immutable conditions to which the living and the dead must alike submit. On the contrary, their vital force pervaded the organism in all its parts. It was an active and energetic opponent of the laws of physics and chemistry. It maintained its own existence not by obeying but by defying them ; and though destined to be finally overcome in the separate campaigns of which each individual living creature is the scene, yet like some guerilla chieftain it was defeated here only to reappear there with unabated confidence and apparently undiminished force.

This attitude of mind checked the advance of knowledge. Difficulty could be evaded by a verbal formula of explanation which in fact explained nothing. If the mechanical, or physical, or chemical causes of a phenomenon did not lie obviously upon the surface, the investigator was tempted to forego the toil of searching for them below ; it was easier to say that the vital force was the cause of the discrepancy, and that it was

hopeless to attempt to account for the action of a principle which was incomprehensible in its nature.

For the physicist the danger is no less serious though it lies in a somewhat different direction. At present he is checked in his theories by the necessity of making them agree with a comparatively small number of fundamental hypotheses. If this check were removed his fancy might run riot in the wildest speculations, which would be held to be legitimate if only they led to formulæ in harmony with facts. But the very habit of regarding the end as everything, and the means by which it was attained as unimportant, would prevent the discovery of those fragments of truth which can only be uncovered by the painful process of trying to make inconsistent theories agree, and using all facts, however remote, as the tests of our central generalisation.

'Science,' said Helmholtz, 'Science, whose very object it is to comprehend Nature, must start with the assumption that Nature is comprehensible.' And again : 'The first principle of the investigator of Nature is to assume that Nature is intelligible to us, since otherwise it would be foolish to attempt the investigation at all.' These axioms do not assume that all the secrets of the universe will ultimately be laid bare, but that a search for them is hopeless if we undertake the quest with the conviction that it will be in vain. As applied to life they do not deny that in living matter something may be hidden which neither physics nor chemistry can explain, but they assert that the action of physical and chemical forces in living bodies can never be understood, if at every difficulty and at every check in our investigations we desist from further attempts in the belief that the laws of physics and chemistry have been interfered with by an incomprehensible vital force. As applied to physics and chemistry they do not mean that all the phenomena of life and death will ultimately be included in some simple and self-sufficing mechanical theory ; they do mean that we are not to sit down contented with paradoxes such as that the same thing can fill both a large space and a little one ; that matter can act where it is not, and the like, if by some reasonable hypothesis, capable of being tested by experiment, we can avoid the acceptance of these absurdities. Something will have been gained if the more obvious difficulties are removed, even if we have to admit that in the background there is much that we cannot grasp.

The Limits of Physical Theories.

And this brings me to my last point. It is a mistake to treat physical theories in general, and the atomic theory in particular, as though they were parts of a scheme which has failed if it leaves anything unexplained, which must be carried on indefinitely on exactly the same principles, whether the ultimate results are, or are not, repugnant to common sense.

Physical theories begin at the surface with phenomena which directly

affect our senses. When they are used in the attempt to penetrate deeper into the secrets of Nature it is more than probable that they will meet with insuperable barriers, but this fact does not demonstrate that the fundamental assumptions are false, and the question as to whether any particular obstacle will be for ever insuperable can rarely be answered with certainty.

Those who belittle the ideas which have of late governed the advance of scientific theory too often assume that there is no alternative between the opposing assertions that atoms and the ether are mere figments of the scientific imagination, or that, on the other hand, a mechanical theory of the atoms and of the ether, which is now confessedly imperfect, would, if it could be perfected, give us a full and adequate representation of the underlying realities.

For my own part I believe that there is a *via media*.

A man peering into a darkened room, and describing what he thinks he sees, may be right as to the general outline of the objects he discerns, wrong as to their nature and their precise forms. In his description fact and fancy may be blended, and it may be difficult to say where the one ends and the other begins; but even the fancies will not be worthless if they are based on a fragment of truth, which will prevent the explorer from walking into a looking-glass or stumbling over the furniture. He who saw 'men as trees walking' had at least a perception of the fundamental fact that something was in motion around him.

And so, at the beginning of the twentieth century, we are neither forced to abandon the claim to have penetrated below the surface of Nature, nor have we, with all our searching, torn the veil of mystery from the world around us.

The range of our speculations is limited both in space and time: in space, for we have no right to claim, as is sometimes done, a knowledge of the 'infinite universe'; in time, for the cumulative effects of actions which might pass undetected in the short span of years of which we have knowledge, may, if continued long enough, modify our most profound generalisations. If some such theory as the vortex-atom theory were true, the faintest trace of viscosity in the primordial medium would ultimately destroy matter of every kind. It is thus a duty to state what we believe we know in the most cautious terms, but it is equally a duty not to yield to mere vague doubts as to whether we can know anything.

If no other conception of matter is possible than that it consists of distinct physical units—and no other conception has been formulated which does not blur what are otherwise clear and definite outlines—if it is certain, as it is, that vibrations travel through space which cannot be propagated by matter, the two foundations of physical theory are well and truly laid. It may be granted that we have not yet framed a consistent image either of the nature of the atoms or of the ether in which they exist; but I have tried to show that in spite of the

tentative nature of some of our theories, in spite of many outstanding difficulties, the atomic theory unifies so many facts, simplifies so much that is complicated, that we have a right to insist—at all events till an equally intelligible rival hypothesis is produced—that the main structure of our theory is true ; that atoms are not merely helps to puzzled mathematicians, but physical realities.

British Association for the Advancement of Science.

BELFAST, 1902.

ADDRESS

BY

PROFESSOR JAMES DEWAR, M.A., LL.D., D.Sc., F.R.S.
PRESIDENT.

THE members of an Association whose studies involve perpetual contemplation of settled law and ordered evolution, whose objects are to seek patiently for the truth of things and to extend the dominion of man over the forces of nature, are even more deeply pledged than other men to loyalty to the Crown and the Constitution which procure for them the essential conditions of calm security and social stability. I am confident that I express the sentiments of all now before me when I say that to our loyal respect for his high office we add a warmer feeling of loyalty and attachment to the person of our Gracious Sovereign. It is the peculiar felicity of the British Association that, since its foundation seventy-one years ago, it has always been easy and natural to cherish both these sentiments, which indeed can never be dissociated without peril. At this, our second meeting held under the present reign, these sentiments are realised all the more vividly, because, in common with the whole empire, we have recently passed through a period of acute apprehension, followed by the uplifting of a national deliverance. The splendid and imposing coronation ceremony which took place just a month ago was rendered doubly impressive both for the King and his people by the universal consciousness that it was also a service of thanksgiving for escape from imminent peril. In offering to His Majesty our most hearty congratulations upon his singularly rapid recovery from a dangerous illness, we rejoice to think that the nation has received gratifying evidence of the vigour of his constitution, and may, with confidence more assured than before, pray that he may have length of happy and prosperous days. No one in his wide dominions is more competent than the King to realise how much he owes, not only to the skill of his surgeons, but also to the equipment which has been placed in their hands as the combined result of scientific investigation in many and diverse directions. He has already displayed a profound and sagacious interest in the discovery of methods for dealing with some of the most intractable maladies that still baffle scientific penetration; nor can we doubt that this interest extends to other forms of scientific investigation, more directly connected with the amelioration

of the lot of the healthy than with the relief of the sick. Heredity imposes obligations and also confers aptitude for their discharge. If His Majesty's royal mother throughout her long and beneficent reign set him a splendid example of devotion to the burdensome labours of State which must necessarily absorb the chief part of his energies, his father no less clearly indicated the great part he may play in the encouragement of science. Intelligent appreciation of scientific work and needs is not less but more necessary in the highest quarters to-day than it was forty-three years ago, when His Royal Highness the Prince Consort brought the matter before this Association in the following memorable passage in his Presidential Address: 'We may be justified, however, in hoping that by the gradual diffusion of science and its increasing recognition as a principal part of our national education, the public in general, no less than the legislature and the State, will more and more recognise the claims of science to their attention; so that it may no longer require the begging box, but speak to the State like a favoured child to its parent, sure of his paternal solicitude for its welfare; that the State will recognise in science one of its elements of strength and prosperity, to protect which the clearest dictates of self-interest demand.' Had this advice been seriously taken to heart and acted upon by the rulers of the nation at the time, what splendid results would have accrued to this country! We should not now be painfully groping in the dark after a system of national education. We should not be wasting money, and time more valuable than money, in building imitations of foreign educational superstructures before having put in solid foundations. We should not be hurriedly and distractedly casting about for a system of tactics after confrontation with the disciplined and co-ordinated forces of industry and science led and directed by the rulers of powerful States. Forty-three years ago we should have started fair had the Prince Consort's views prevailed. As it is, we have lost ground which it will tax even this nation's splendid reserves of individual initiative to recover. Although in this country the king rules, but does not govern, the Constitution and the structure of English society assure to him a very potent and far-reaching influence upon those who do govern. It is hardly possible to overrate the benefits that may accrue from his intelligent and continuous interest in the great problem of transforming his people into a scientifically educated nation. From this point of view we may congratulate ourselves that the heir to the Crown, following his family traditions, has already deduced from his own observations in different parts of the empire some very sound and valuable conclusions as to the national needs at the present day.

Griffith—Gilbert—Cornu.

The saddest yet the most sacred duty falling to us on such an occasion as the present is to pay our tribute to the memory of old comrades and fellow-workers whom we shall meet no more. We miss to-day a figure

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that has been familiar, conspicuous, and always congenial at the meetings of the British Association during the last forty years. Throughout the greater part of that period Mr. George Griffith discharged the onerous and often delicate duties of the assistant general secretary, not only with conscientious thoroughness and great ability, but also with urbanity, tact, and courtesy that endeared him to all. His years sat lightly upon him, and his undiminished alertness and vigour caused his sudden death to come upon us all with a shock of surprise as well as of pain and grief. The British Association owes him a debt of gratitude which must be so fully realised by every regular attender of our meetings that no poor words of mine are needed to quicken your sense of loss, or to add to the poignancy of your regret.

The British Association has to deplore the loss from among us of Sir Joseph Gilbert, a veteran who continued to the end of a long life to pursue his important and beneficent researches with untiring energy. The length of his services in the cause of science cannot be better indicated than by recalling the fact that he was one of the six past Presidents boasting fifty years' membership whose jubilee was celebrated by the Chemical Society in 1898. He was in fact an active member of that Society for over sixty years. Early in his career he devoted himself to a most important but at that time little cultivated field of research. He strove with conspicuous success to place the oldest of industries on a scientific basis, and to submit the complex conditions of agriculture to a systematic analysis. He studied the physiology of plant life in the open air, not with the object of penetrating the secrets of structure, but with the more directly utilitarian aim of establishing the conditions of successful and profitable cultivation. By a long series of experiments alike well conceived and laboriously carried out, he determined the effects of variation in soil, and its chemical treatment—in short, in all the unknown factors with which the farmer previously had to deal according to empirical and local rules, roughly deduced from undigested experience by uncritical and rudimentary processes of inference. Gilbert had the faith, the insight, and the courage to devote his life to an investigation so difficult, so unpromising, and so unlikely to bring the rich rewards attainable by equal diligence in other directions, as to offer no attraction to the majority of men. The tabulated results of the Rothamsted experiments remain as a benefaction to mankind and a monument of indomitable and disinterested perseverance.

It is impossible for me in this place to offer more than the barest indication of the great place in contemporary science that has been vacated by the lamented death of Professor Alfred Cornu, who so worthily upheld the best traditions of scientific France. He was gifted in a high degree with the intellectual lucidity, the mastery of form, and the perspicuous method which characterise the best exponents of French thought in all departments of study. After a brilliant career as a student, he was chosen at the early age of twenty-six to fill one of the enviable positions

more numerous in Paris than in London, the Professorship of Physics at the École Polytechnique. In that post, which he occupied to the end of his life, he found what is probably the ideal combination for a man of science—leisure and material equipment for original research, together with that close and stimulating contact with practical affairs afforded by his duties as teacher in a great school, almost ranking as a department of State. Cornu was admirable alike in the use he made of his opportunities and in his manner of discharging his duties. He was at once a great investigator and a great teacher. I shall not even attempt a summary, which at the best must be very imperfect, of his brilliant achievements in optics, the study of his predilection, in electricity, in acoustics, and in the field of physics generally. As a proof of the great estimation in which he was held, it is sufficient to remind you that he had filled the highest presidential offices in French scientific societies, and that he was a foreign member of our Royal Society and a recipient of its Rumford medal. In this country he had many friends, attracted no less by his personal and social qualities than by his commanding abilities. Some of those here present may remember his appearance a few years ago at the Royal Institution, and more recently his delivery of the Rede Lecture at Cambridge, when the University conferred upon him the honorary degree of Doctor of Science. His death has inflicted a heavy blow upon our generation, upon France, and upon the world.

The Progress of Belfast.

A great man has observed that the 'intelligent anticipation of events before they occur' is a factor of some importance in human affairs. One may suppose that intelligent anticipation had something to do with the choice of Belfast as the meeting-place of the British Association this year. Or, if it had not, then it must be admitted that circumstances have conspired, as they occasionally do, to render the actual selection peculiarly felicitous. Belfast has perennial claims, of a kind that cannot easily be surpassed, to be the scene of a great scientific gathering—claims founded upon its scientific traditions and upon the conspicuous energy and success with which its citizens have prosecuted in various directions the application of science to the purposes of life. It is but the other day that the whole nation deplored at the grave of Lord Dufferin the loss of one of the most distinguished and most versatile public servants of the age. That great statesman and near neighbour of Belfast was a typical expression of the qualities and the spirit which have made Belfast what it is, and have enabled Ireland, in spite of all drawbacks, to play a great part in the Empire. I look round on your thriving and progressive city giving evidence of an enormous aggregate of industrial efforts intelligently organised and directed for the building up of a sound social fabric. I find that your great industries are interlinked and interwoven with the whole economic framework of the Empire, and that you are silently and irresistibly compelled to harmonious co-operation by practical considerations

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acting upon the whole community. It is here that I look for the real Ireland, the Ireland of the future. We cannot trace with precision the laws that govern the appearance of eminent men, but we may at least learn from history that they do not spring from every soil. They do not appear among decadent races or in ages of retrogression. They are the fine flower of the practical intellect of the nation working studiously and patiently in accordance with the great laws of conduct. In the manifold activities of Belfast we have a splendid manifestation of individual energy working necessarily, even if not altogether consciously, for the national good. In great Irishmen like Lord Dufferin and Lord Roberts, giving their best energies for the defence of the nation by diplomacy or by war, we have complementary evidence enough to reassure the most timid concerning the real direction of Irish energies and the vital nature of Irish solidarity with the rest of the Empire.

Belfast has played a prominent part in a transaction of a somewhat special and significant kind, which has proved not a little confusing and startling to the easy-going public. The significance of the shipping combination lies in the light it throws on the conditions and tendencies which make such things possible, if not even inevitable. It is an event forcibly illustrating the declaration of His Royal Highness the Prince of Wales, that the nation must 'wake up' if it hopes to face its growing responsibilities. Belfast may plead with some justice that it, at least, has never gone to sleep. In various directions an immense advance has been effected during the twenty-eight years that have elapsed since the last visit of the British Association. Belfast has become first a city and then a county, and now ranks as one of the eight largest cities in the United Kingdom. Its municipal area has been considerably extended, and its population has increased by something like 75 per cent. It has not only been extended, but improved and beautified in a manner which very few places can match, and which probably none can surpass. Fine new thoroughfares, adorned with admirable public institutions, have been run through areas once covered with crowded and squalid buildings. Compared with the early fifties, when iron shipbuilding was begun on a very modest scale, the customs collected at the port have increased tenfold. Since the introduction of the power-loom, about 1850, Belfast has distanced all rivals in the linen industry, which continues to flourish notwithstanding the fact that most of the raw material is now imported, instead of being produced, as in former times, in Ulster. Extensive improvements have been carried out in the port at a cost of several millions, and have been fully justified by a very great expansion of trade. These few bare facts suffice to indicate broadly the immense strides taken by Belfast in the last two decades. For an Association that exists for the advancement of science it is stimulating and encouraging to find itself in the midst of a vigorous community, successfully applying knowledge to the ultimate purpose of all human effort, the amelioration of the common lot by an ever-increasing mastery of the powers and resources of Nature.

Tyndall and Evolution.

The Presidential Address delivered by Tyndall in this city twenty-eight years ago will always rank as an epoch-making deliverance. Of all the men of the time, Tyndall was one of the best equipped for the presentation of a vast and complicated scientific subject to the mass of his fellow-men. Gifted with the powers of a many-sided original investigator, he had at the same time devoted much of his time to an earnest study of philosophy, and his literary and oratorical powers, coupled with a fine poetic instinct, were qualifications which placed him in the front rank of the scientific representatives of the later Victorian epoch, and constituted him an exceptionally endowed exponent of scientific thought. In the Belfast discourse Tyndall dealt with the changing aspects of the long unsettled horizon of human thought, at last illuminated by the sunrise of the doctrine of evolution. The consummate art with which he marshalled his scientific forces for the purpose of effecting conviction of the general truth of the doctrine has rarely been surpassed. The courage, the lucidity, the grasp of principles, the moral enthusiasm with which he treated his great theme, have powerfully aided in effecting a great intellectual conquest, and the victory assuredly ought to engender no regrets.

Tyndall's views as a strenuous supporter and believer in the theory of evolution were naturally essentially optimistic. He had no sympathy with the lugubrious pessimistic philosophy whose disciples are for ever intent on administering rebuke to scientific workers by reminding them that, however much knowledge man may have acquired, it is as nothing compared with the immensity of his ignorance. That truth is indeed never adequately realised except by the man of science, to whom it is brought home by repeated experience of the fact that his most promising excursions into the unknown are invariably terminated by barriers which, for the time at least, are insurmountable. He who has never made such excursions with patient labour may indeed prattle about the vastness of the unknown, but he does so without real sincerity or intimate conviction. His tacit, if not his avowed, contention is, that since we can never know all it is not worth while to seek to know more ; and that in the profundity of his ignorance he has the right to people the unexplored spaces with the phantoms of his vain imagining. The man of science, on the contrary, finds in the extent of his ignorance a perpetual incentive to further exertion, and in the mysteries that surround him a continual invitation, nay, more, an inexorable mandate. Tyndall's writings abundantly prove that he had faced the great problems of man's existence with that calm intellectual courage, the lack of which goes very far to explain the nervous dogmatism of nescience. Just because he had done this, because he had, as it were, mapped out the boundaries between what is knowable though not yet known and what must remain for ever unknowable to man, he did not hesitate to place implicit reliance on the progress of which man is

capable, through the exercise of patient and persistent research. In Tyndall's scheme of thought the chief dicta were the strict division of the world of knowledge from that of emotion, and the lifting of life by throwing overboard the malign residuum of dogmatism, fanaticism, and intolerance, thereby stimulating and nourishing a plastic vigour of intellect. His cry was 'Commotion before stagnation, the leap of the torrent before the stillness of the swamp.'

His successors have no longer any need to repeat those significant words, 'We claim and we shall wrest from theology the entire domain of cosmological theory.' The claim has been practically, though often unconsciously, conceded. Tyndall's dictum, 'Every system must be plastic to the extent that the growth of knowledge demands,' struck a note that was too often absent from the heated discussions of days that now seem so strangely remote. His honourable admission that, after all that had been achieved by the developmental theory, 'the whole process of evolution is the manifestation of a power absolutely inscrutable to the intellect of man,' shows how willingly he acknowledged the necessary limits of scientific inquiry. This reservation did not prevent him from expressing the conviction forced upon him by the pressure of intellectual necessity, after exhaustive consideration of the known relations of living things, that matter in itself must be regarded as containing the promise and potency of all terrestrial life. Bacon in his day said very much the same thing: 'He that will know the properties and proceedings of matter should comprehend in his understanding the sum of all things, which have been, which are, and which shall be, although no knowledge can extend so far as to singular and individual beings.' Tyndall's conclusion was at the time thought to be based on a too insecure projection into the unknown, and some even regarded such an expansion of the crude properties of matter as totally unwarranted. Yet Tyndall was certainly no materialist in the ordinary acceptation of the term. It is true his arguments, like all arguments, were capable of being distorted, especially when taken out of their context, and the address became in this way an easy prey for hostile criticism. The glowing rhetoric that gave charm to his discourse and the poetic similes that clothed the dry bones of his close-woven logic were attacked by a veritable broadside of critical artillery. At the present day these would be considered as only appropriate artistic embellishments, so great is the unconscious change wrought in our surroundings. It must be remembered that, while Tyndall discussed the evolutionary problem from many points of view, he took up the position of a practical disciple of Nature dealing with the known experimental and observational realities of physical inquiry. Thus he accepted as fundamental concepts the atomic theory, together with the capacity of the atom to be the vehicle or repository of energy, and the grand generalisation of the conservation of energy. Without the former, Tyndall doubted whether it would be possible to frame a theory of the material universe; and as to the latter he recognised its radical significance in that the ultimate

philosophical issues therein involved were as yet but dimly seen. That such generalisations are provisionally accepted does not mean that science is not alive to the possibility that what may now be regarded as fundamental may in future be superseded or absorbed by a wider generalisation. It is only the poverty of language and the necessity for compendious expression that oblige the man of science to resort to metaphor and to speak of the Laws of Nature. In reality, he does not pretend to formulate any laws for Nature, since to do so would be to assume a knowledge of the inscrutable cause from which alone such laws could emanate. When he speaks of a 'law of Nature' he simply indicates a sequence of events which, so far as his experience goes, is invariable, and which therefore enables him to predict, to a certain extent, what will happen in given circumstances. But, however seemingly bold may be the speculation in which he permits himself to indulge, he does not claim for his best hypothesis more than provisional validity. He does not forget that to-morrow may bring a new experience compelling him to recast the hypothesis of to-day. This plasticity of scientific thought, depending upon reverent recognition of the vastness of the unknown, is oddly made a matter of reproach by the very people who harp upon the limitations of human knowledge. Yet the essential condition of progress is that we should generalise to the best of our ability from the experience at command, treat our theory as provisionally true, endeavour to the best of our power to reconcile with it all the new facts we discover, and abandon or modify it when it ceases to afford a coherent explanation of new experience. That procedure is far as are the poles asunder from the presumptuous attempt to travel beyond the study of secondary causes. Any discussion as to whether matter or energy was the true reality would have appeared to Tyndall as a futile metaphysical disputation, which, being completely dissociated from verified experience, would lead to nothing. No explanation was attempted by him of the origin of the bodies we call elements, nor how some of such bodies came to be compounded into complex groupings and built up into special structures with which, so far as we know, the phenomena characteristic of life are invariably associated. The evolutionary doctrine leads us to the conclusion that life, such as we know it, has only been possible during a short period of the world's history, and seems equally destined to disappear in the remote future; but it postulates the existence of a material universe endowed with an infinity of powers and properties, the origin of which it does not pretend to account for. The enigma at both ends of the scale Tyndall admitted, and the futility of attempting to answer such questions he fully recognised. Nevertheless, Tyndall did not mean that the man of science should be debarred from speculating as to the possible nature of the simplest forms of matter or the mode in which life may have originated on this planet. Lord Kelvin, in his Presidential Address, put the position admirably when he said 'Science is bound by the everlasting law of honour to face fearlessly every problem that can fairly be presented to it. If a probable solution consistent with the ordinary course

of Nature can be found, we must not invoke an abnormal act of Creative Power'; and in illustration he forthwith proceeded to express his conviction that from time immemorial many worlds of life besides our own have existed, and that 'it is not an unscientific hypothesis that life originated on this earth through the moss-grown fragments from the ruins of another world.' In spite of the great progress made in science, it is curious to notice the occasional recrudescence of metaphysical dogma. For instance, there is a school which does not hesitate to revive ancient mystifications in order to show that matter and energy can be shattered by philosophical arguments, and have no objective reality. Science is at once more humble and more reverent. She confesses her ignorance of the ultimate nature of matter, of the ultimate nature of energy, and still more of the origin and ultimate synthesis of the two. She is content with her patient investigation of secondary causes, and glad to know that since Tyndall spoke in Belfast she has made great additions to the knowledge of general molecular mechanism, and especially of synthetic artifice in the domain of organic chemistry, though the more exhaustive acquaintance gained only forces us the more to acquiesce in acknowledging the inscrutable mystery of matter. Our conception of the power and potency of matter has grown in little more than a quarter of a century to much more imposing dimensions, and the outlook for the future assuredly suggests the increasing acceleration of our rate of progress. For the impetus he gave to scientific work and thought, and for his fine series of researches chiefly directed to what Newton called the more secret and noble works of Nature within the corpuscles, the world owes Tyndall a debt of gratitude. It is well that his memory should be held in perennial respect, especially in the land of his birth.

The Endowment of Education.

These are days of munificent benefactions to science and education, which however are greater and more numerous in other countries than in our own. Splendid as they are, it may be doubted, if we take into account the change in the value of money, the enormous increase of population, and the utility of science to the builders of colossal fortunes, whether they bear comparison with the efforts of earlier days. But the habit of endowing science was so long in practical abeyance that every evidence of its resumption is matter for sincere congratulation. Mr. Cecil Rhodes has dedicated a very large sum of money to the advancement of education, though the means he has chosen are perhaps not the most effective. It must be remembered that his aims were political as much as educational. He had the noble and worthy ambition to promote enduring friendship between the great English-speaking communities of the world, and knowing the strength of college ties he conceived that this end might be greatly furthered by bringing together at an English university the men who would presumably have much to do in later life with the influencing of opinion, or even with the direction of policy. It has

been held by some a striking tribute to Oxford that a man but little given to academic pursuits or modes of thought should think it a matter of high importance to bring men from our colonies or even from Germany, to submit to the formative influences of that ancient seat of learning. But this is perhaps reading Mr. Rhodes backwards. He showed his affectionate recollection of his college days by his gift to Oriel. But, apart from the main idea of fostering good relations between those who will presumably be influential in England, in the colonies, and in the United States, Mr Rhodes was probably influenced also by the hope that the influx of strangers would help to broaden Oxford notions and to procure revision of conventional arrangements.

Dr. Andrew Carnegie's endowment of Scottish universities, as modified by him in deference to expert advice, is a more direct benefit to the higher education. For while Mr. Rhodes has only enabled young men to get what Oxford has to give, Dr. Carnegie has also enabled his trustees powerfully to augment and improve the teaching equipment of the universities themselves. At the same time he has provided as far as possible for the enduring usefulness of his money. His trustees form a permanent body external to the universities, which, while possessing no power of direct control, must always, as holder of the purse-strings, be in a position to offer independent and weighty criticisms. More recently Dr. Carnegie has devoted an equal sum of ten million dollars to the foundation of a Carnegie Institution in Washington. Here again he has been guided by the same ideas. He has neither founded a university nor handed over the money to any existing university. He has created a permanent trust charged with the duty of watching educational efforts and helping them from the outside according to the best judgment that can be formed in the circumstances of the moment. Its aims are to be—to promote original research ; to discover the exceptional man in every department of study, whether inside or outside of the schools, and to enable him to make his special study his life-work ; to increase facilities for higher education ; to aid and stimulate the universities and other educational institutions ; to assist students who may prefer to study at Washington ; and to ensure prompt publication of scientific discoveries. The general purpose of the founder is to secure, if possible, for the United States leadership in the domain of discovery and the utilisation of new forces for the benefit of man. Nothing will more powerfully further this end than attention to the injunction to lay hold of the exceptional man whenever and wherever he may be found, and, having got him, to enable him to carry on the work for which he seems specially designed. That means, I imagine, a scouring of the old world, as well as of the new, for the best men in every department of study—in fact, an assiduous collecting of brains similar to the collecting of rare books and works of art which Americans are now carrying on in so lavish a manner. As in diplomacy and war, so in science, we owe our reputation, and no small part of our prosperity, to exceptional men ; and that we do not enjoy these things in fuller measure we owe to our

lack of an army of well-trained ordinary men capable of utilising their ideas. Our exceptional men have too often worked in obscurity, without recognition from a public too imperfectly instructed to guess at their greatness, without assistance from a State governed largely by dialecticians, and without help from academic authorities hidebound by the pedantries of medieval scholasticism. For such men we have to wait upon the will of Heaven. Even Dr. Carnegie will not always find them when they are wanted. But what can be done in that direction will be done by institutions like Dr. Carnegie's, and for the benefit of the nation that possesses them in greatest abundance and uses them most intelligently. When contemplating these splendid endowments of learning, it occurred to me that it would be interesting to find out exactly what some definite quantity of scientific achievement has cost in hard cash. In an article by Carl Snyder in the January number of the 'North American Review,' entitled 'America's Inferior Place in the Scientific World,' I found the statement that 'it would be hardly too much to say that during the hundred years of its existence the Royal Institution alone has done more for English science than all of the English universities put together. This is certainly true with regard to British industry, for it was here that the discoveries of Faraday were made.' I was emboldened by this estimate from a distant and impartial observer to do, what otherwise I might have shrunk from doing, and to take the Royal Institution—after all, the foundation of an American citizen, Count Rumford—as the basis of my inquiry. The work done at the Royal Institution during the past hundred years is a fairly definite quantity in the mind of every man really conversant with scientific affairs. I have obtained from the books accurate statistics of the total expenditure on experimental inquiry and public demonstrations for the whole of the nineteenth century. The items are :

	£
Professors' Salaries—Physics and Chemistry .	54,600
Laboratory Expenditure	24,430
Assistants' Salaries	21,590
Total for one hundred years	£100,620

In addition, the members and friends of the Institution have contributed to a fund for exceptional expenditure for Experimental Research the sum of 9,580*l.* It should also be mentioned that a Civil List pension of 300*l.* was granted to Faraday in 1853, and was continued during twenty-seven years of active work and five years of retirement. Thirty-two years in all, at 300*l.* a year, make a sum of 9,600*l.*, representing the national donation, which, added to the amount of expenditure just stated, brings up the total cost of a century of scientific work in the laboratories of the Royal Institution, together with public demonstrations, to 119,800*l.*, or an average of 1,200*l.* per annum. I think if you recall the names and achievements of Young, Davy, Faraday, and Tyndall, you will come to the

conclusion that the exceptional man is about the cheapest of natural products. It is a popular fallacy that the Royal Institution is handsomely endowed. On the contrary, it has often been in financial straits; and since its foundation by Count Rumford its only considerable bequests have been one from Thomas G. Hodgkins, an American citizen, for Experimental Research, and that of John Fuller for endowing with 95*l.* a year the chairs of Chemistry and Physiology. In this connection the Davy-Faraday Laboratory, founded by the liberality of Dr. Ludwig Mond, will naturally occur to many minds. But though affiliated to the Royal Institution, with, I hope, reciprocal indirect advantages, that Laboratory is financially independent and its endowments are devoted to its own special purpose, which is to provide opportunity to prosecute independent research for worthy and approved applicants of all nationalities. The main reliance of the Royal Institution has always been, and still remains, upon the contributions of its members, and upon corresponding sacrifices in the form of time and labour by its professors. It may be doubted whether we can reasonably count upon a succession of scientific men able and willing to make sacrifices which the conditions of modern life tend to render increasingly burdensome. Modern science is in fact in something of a dilemma. Devotion to abstract research upon small means is becoming always harder to maintain, while at the same time the number of wealthy independent searchers after truth and patrons of science of the style of Joule, Spottiswoode, and De la Rue is apparently becoming smaller. The installations required by the refinements of modern science are continually becoming more costly, so that upon all grounds it would appear that without endowments of the kind provided by Dr. Carnegie the outlook for disinterested research is rather dark. On the other hand, these endowments, unless carefully administered, might obviously tend to impair the single-minded devotion to the search after truth for its own sake, to which science has owed almost every memorable advance made in the past. The Carnegie Institute will dispose in a year of as much money as the members of the Royal Institution have expended in a century upon its purely scientific work. It will at least be interesting to note how far the output of high-class scientific work corresponds to the hundredfold application of money to its production. Nor will it be of less interest to the people of this country to observe the results obtained from that moiety of Dr. Carnegie's gift to Scotland which is to be applied to the promotion of scientific research.

Applied Chemistry, English and Foreign.

The Diplomatic and Consular reports published from time to time by the Foreign Office are usually too belated to be of much use to business men, but they sometimes contain information concerning what is done in foreign countries which affords food for reflection. One of these reports, issued a year ago, gives a very good account of the German arrangements

and provisions for scientific training, and of the enormous commercial demand for the services of men who have passed successfully through the universities and Technical High Schools, as well as of the wealth that has accrued to Germany through the systematic application of scientific proficiency to the ordinary business of life.

Taking these points in their order, I have thought it a matter of great interest to obtain a comparative view of chemical equipment in this country and in Germany, and I am indebted to Professor Henderson of Glasgow, who last year became the secretary of a committee of this Association of which Professor Armstrong is chairman, for statistics referring to this country, which enable a comparison to be broadly made. The author of the consular report estimates that in 1901 there were 4,500 trained chemists employed in German works, the number having risen to this point from 1,700 employed twenty-five years earlier. It is difficult to give perfectly accurate figures for this country, but a liberal estimate places the number of works chemists at 1,500, while at the very outside it cannot be put higher than somewhere between 1,500 and 2,000. In other words, we cannot show in the United Kingdom, notwithstanding the immense range of the chemical industries in which we once stood prominent, more than one-third of the professional staff employed in Germany. It may perhaps be thought or hoped that we make up in quality for our defect in quantity, but unfortunately this is not the case. On the contrary, the German chemists are, on the average, as superior in technical training and acquirements as they are numerically. Details are given in the report of the training of 633 chemists employed in German works. Of these, 69 per cent. hold the degree of Ph.D., about 10 per cent. hold the diploma of a Technical High School, and about 5 per cent. hold both qualifications. That is to say 84 per cent. have received a thoroughly systematic and complete chemical training, and 74 per cent. of these add the advantages of a university career. Compare with this the information furnished by 500 chemists in British works. Of these only 21 per cent. are graduates, while about 10 per cent. hold the diploma of a college. Putting the case as high as we can, and ignoring the more practical and thorough training of the German universities, which give their degrees for work done, and not for questions asked and answered on paper, we have only 31 per cent. of systematically trained chemists against 84 per cent. in German works. It ought to be mentioned that about 21 per cent. of the 500 are Fellows or Associates of the Institute of Chemistry, whatever that may amount to in practice, but of these a very large number have already been accounted for under the heads of graduates and holders of diplomas. These figures, which I suspect are much too favourable on the British side, unmistakably point to the prevalence among employers in this country of the antiquated adherence to rule of thumb, which is at the root of much of the backwardness we have to deplore. It hardly needs to be pointed out to such an audience as the present that chemists who are neither graduates of a university, nor holders of a diploma from a

technical college, may be competent to carry on existing processes according to traditional methods, but are very unlikely to effect substantial improvements, or to invent new and more efficient processes. I am very far from denying that here and there an individual may be found whose exceptional ability enables him to triumph over all defects of training. But in all educational matters it is the average man whom we have to consider, and the average ability which we have to develop. Now, to take the second point—the actual money value of the industries carried on in Germany by an army of workers both quantitatively and qualitatively so superior to our own. The Consular report estimates the whole value of German chemical industries at not less than fifty millions sterling per annum. These industries have sprung up within the last seventy years, and have received enormous expansion during the last thirty. They are, moreover, very largely founded upon basic discoveries made by English chemists, but never properly appreciated or scientifically developed in the land of their birth. I will place before you some figures showing the growth of a single firm engaged in a single one of these industries—the utilisation of coal tar for the production of drugs, perfumes, and colouring-matters of every conceivable shade. The firm of Friedrich Bayer & Co. employed in 1875, 119 workmen. The number has more than doubled itself every five years, and in May of this year that firm employed 5,000 workmen, 160 chemists, 260 engineers and mechanics, and 680 clerks. For many years past it has regularly paid 18 per cent. on the ordinary shares, which this year has risen to 20 per cent.; and in addition, in common with other and even larger concerns in the same industry, has paid out of profits for immense extensions usually charged to capital account. There is one of these factories, the works and plant of which stand in the books at 1,500,000*l.*, while the money actually sunk in them approaches to 5,000,000*l.* In other words, the practical monopoly enjoyed by the German manufacturers enables them to exact huge profits from the rest of the world, and to establish a position which, financially as well as scientifically, is almost unassailable. I must repeat that the fundamental discoveries upon which this gigantic industry is built were made in this country, and were practically developed to a certain extent by their authors. But in spite of the abundance and cheapness of the raw material, and in spite of the evidence that it could be most remuneratively worked up, these men founded no school and had practically no successors. The colours they made were driven out of the field by newer and better colours made from their stuff by the development of their ideas, but these improved colours were made in Germany and not in England. Now what is the explanation of this extraordinary and disastrous phenomenon? I give it in a word—want of education. We had the material in abundance when other nations had comparatively little. We had the capital, and we had the brains, for we originated the whole thing. But we did not possess the diffused education without which the ideas of men of genius cannot fructify beyond

the limited scope of an individual. I am aware that our patent laws are sometimes held responsible. Well, they are a contributory cause ; but it must be remembered that other nations with patent laws as protective as could be desired have not developed the colour industry. The patent laws have only contributed in a secondary degree, and if the patent laws have been bad the reason for their badness is again want of education. Make them as bad as you choose, and you only prove that the men who made them, and the public whom these men try to please, were misled by theories instead of being conversant with fact and logic. But the root of the mischief is not in the patent laws or in any legislation whatever. It is in the want of education among our so-called educated classes, and secondarily among the workmen on whom these depend. It is in the abundance of men of ordinary plodding ability, thoroughly trained and methodically directed, that Germany at present has so commanding an advantage. It is the failure of our schools to turn out, and of our manufacturers to demand, men of this kind, which explains our loss of some valuable industries and our precarious hold upon others. Let no one imagine for a moment that this deficiency can be remedied by any amount of that technical training which is now the fashionable nostrum. It is an excellent thing, no doubt, but it must rest upon a foundation of general training. Mental habits are formed for good or evil long before men go to the technical schools. We have to begin at the beginning : we have to train the population from the first to think correctly and logically, to deal at first hand with facts, and to evolve, each one for himself, the solution of a problem put before him, instead of learning by rote the solution given by somebody else. There are plenty of chemists turned out, even by our Universities, who would be of no use to Bayer & Co. They are chockfull of formulæ, they can recite theories, and they know textbooks by heart ; but put them to solve a new problem, freshly arisen in the laboratory, and you will find that their learning is all dead. It has not become a vital part of their mental equipment, and they are floored by the first emergence of the unexpected. The men who escape this mental barrenness are men who were somehow or other taught to think long before they went to the university. To my mind, the really appalling thing is not that the Germans have seized this or the other industry, or even that they may have seized upon a dozen industries. It is that the German population has reached a point of general training and specialised equipment which it will take us two generations of hard and intelligently directed educational work to attain. It is that Germany possesses a national weapon of precision which must give her an enormous initial advantage in any and every contest depending upon disciplined and methodised intellect.

History of Cold and the Absolute Zero.

It was Tyndall's good fortune to appear before you at a moment when a fruitful and comprehensive idea was vivifying the whole domain of scientific thought. At the present time no such broad generalisation presents itself for discussion, while on the other hand the number of specialised studies has enormously increased. Science is advancing in so broad a front by the efforts of so great an army of workers that it would be idle to attempt within the limits of an address to the most indulgent of audiences anything like a survey of chemistry alone. But I have thought it might be instructive, and perhaps not uninteresting, to trace briefly in broad outline the development of that branch of study with which my own labours have been recently more intimately connected—a study which I trust I am not too partial in thinking is as full of philosophical interest as of experimental difficulty. The nature of heat and cold must have engaged thinking men from the very earliest dawn of speculation upon the external world; but it will suffice for the present purpose if, disregarding ancient philosophers and even medieval alchemists, we take up the subject where it stood after the great revival of learning, and as it was regarded by the father of the 'inductive method. That this was an especially attractive subject to Bacon is evident from the frequency with which he recurs to it in his different works, always with lamentation over the inadequacy of the means at disposal for obtaining a considerable degree of cold. Thus in the chapter in the *Natural History*, '*Sylva Sylvarum*,' entitled '*Experiments in consort touching the production of cold*,' he says, '*The production of cold is a thing very worthy of the inquisition both for the use and the disclosure of causes. For heat and cold are nature's two hands whereby she chiefly worketh, and heat we have in readiness in respect of the fire, but for cold we must stay till it cometh or seek it in deep caves or high mountains, and when all is done we cannot obtain it in any great degree, for furnaces of fire are far hotter than a summer sun, but vaults and hills are not much colder than a winter's frost.*' The great Robert Boyle was the first experimentalist who followed up Bacon's suggestions. In 1682 Boyle read a paper to the Royal Society on '*New Experiments and Observations touching Cold, or an Experimental History of Cold*,' published two years later in a separate work. This is really a most complete history of everything known about cold up to that date, but its great merit is the inclusion of numerous experiments made by Boyle himself on frigorific mixtures, and the general effects of such upon matter. The agency chiefly used by Boyle in the conduct of his experiments was the glaciating mixture of snow or ice and salt. In the course of his experiments he made many important observations. Thus he observed that the salts which did not help the snow or ice to dissolve faster gave no effective freezing. He showed that water in becoming ice expands by about one-ninth of its volume, and bursts gun-barrels. He attempted to counteract the expansion and prevent freezing by completely

filling a strong iron ball with water before cooling ; anticipating that it might burst the bottle by the stupendous force of expansion, or that if it did not, then the ice produced might under the circumstances be heavier than water. He speculated in an ingenious way on the change of water into ice. Thus he says, 'If cold be but a privation of heat through the recess of that ethereal substance which agitated the little eel-like particles of the water and thereby made them compose a fluid body, it may easily be conceived that they should remain rigid in the postures in which the ethereal substance quitted them, and thereby compose an unfluid body like ice ; yet how these little eels should by that recess acquire as strong an endeavour outwards as if they were so many little springs and expand themselves with so stupendous a force, is that which does not so readily appear.' The greatest degree of adventitious cold Boyle was able to produce did not make air exposed to its action lose a full tenth of its own volume, so that, in his own words, the cold does not 'weaken the spring by anything near so considerable as one would expect.' After making this remarkable observation and commenting upon its unexpected nature, it is strange Boyle did not follow it up. He questions the existence of a body of its own nature supremely cold, by participating in which all other bodies obtain that quality, although the doctrine of a *primum frigidum* had been accepted by many sects of philosophers ; for, as he says, 'if a body being cold signify no more than its not having its sensible parts so much agitated as those of our sensorium, it suffices that the sun or the fire or some other agent, whatever it were, that agitated more vehemently its parts before, does either now cease to agitate them or agitates them but very remissly, so that till it be determined, whether cold be a positive quality or but a privative it will be needless to contend what particular body ought to be esteemed the *primum frigidum*.' The whole elaborate investigation cost Boyle immense labour, and he confesses that he 'never handled any part of natural philosophy that was so troublesome and full of hardships.' He looked upon his results but as a 'beginning' in this field of inquiry, and for all the trouble and patience expended he consoled himself with the thought of 'men being oftentimes obliged to suffer as much wet and cold and dive as deep to fetch up sponges as to fetch up pearls.' After the masterly essay of Boyle, the attention of investigators was chiefly directed to improving thermometrical instruments. The old air thermometer of Galileo being inconvenient to use, the introduction of fluid thermometers greatly aided the inquiry into the action of heat and cold. For a time great difficulty was encountered in selecting proper fixed points on the scales of such instruments, and this stimulated men like Huygens, Newton, Hooke, and Amontons to suggest remedies and to conduct experiments. By the beginning of the eighteenth century the freezing-point and the boiling-point of water were agreed upon as fixed points, and the only apparent difficulties to be overcome were the selection of the fluid, accurate calibration of the capillary tube of the thermometer, and a general understanding as to scale divisions. It must be confessed

that great confusion and inaccuracy in temperature observations arose from the variety and crudeness of the instruments. This led Amontons in 1702-3 to contribute two papers to the French Academy which reveal great originality in the handling of the subject, and which, strange to say, are not generally known. The first discourse deals with some new properties of the air and the means of accurately ascertaining the temperature in any climate. He regarded heat as due to a movement of the particles of bodies, though he did not in any way specify the nature of the motion involved ; and as the general cause of all terrestrial motion, so that in its absence the earth would be without movement in its smallest parts. The new facts he records are observations on the spring or pressure of air brought about by the action of heat. He shows that different masses of air measured at the same initial spring or pressure, when heated to the boiling-point of water, acquire equal increments of spring or pressure, provided the volume of the gas be kept at its initial value. Further, he proves that if the pressure of the gas before heating be doubled or tripled, then the additional spring or pressure resulting from heating to the boiling-point of water is equally doubled or tripled. In other words, the ratio of the total spring of air at two definite and steady temperatures and at constant volume is a constant, independent of the mass or the initial pressure of the air in the thermometer. These results led to the increased perfection of the air thermometer as a standard instrument, Amontons' idea being to express the temperature at any locality in fractions of the degree of heat of boiling water. The great novelty of the instrument is that temperature is defined by the measurement of the length of a column of mercury. In passing, he remarks that we do not know the extreme of heat and cold, but that he has given the results of experiments which establish correspondences for those who wish to consider the subject. In the following year Amontons contributed to the Academy a further paper extending the scope of the inquiry. He there pointed out more explicitly that as the degrees of heat in his thermometer are registered by the height of a column of mercury, which the heat is able to sustain by the spring of the air, it follows that the extreme cold of the thermometer will be that which reduces the air to have no power of spring. This, he says, will be a much greater cold than what we call 'very cold,' because experiments have shown that if the spring of the air at boiling-point is 73 inches, the degree of heat which remains in the air when brought to the freezing-point of water is still very great, for it can still maintain the spring of $51\frac{1}{2}$ inches. The greatest climatic cold on the scale of units adopted by Amontons is marked 50, and the greatest summer heat 58, the value for boiling water being 73, and the zero being 52 units below the freezing-point. Thus Amontons was the first to recognise that the use of air as a thermometric substance led to the inference of the existence of a zero of temperature, and his scale is nothing else than the absolute one we are now so familiar with. It results from Amontons' experiments that the air would have no

spring left if it were cooled below the freezing-point of water to about $2\frac{1}{2}$ times the temperature range which separates the boiling-point and the freezing-point. In other words, if we adopt the usual centennial difference between these two points of temperature as 100 degrees, then the zero of Amontons' air thermometer is *minus* 240 degrees. This is a remarkable approximation to our modern value for the same point of *minus* 273 degrees. It has to be confessed that Amontons' valuable contributions to knowledge met with that fate which has so often for a time overtaken the work of too-advanced discoverers; in other words, it was simply ignored, or in any case not appreciated by the scientific world either of that time or half a century later. It is not till Lambert, in his work on 'Pyrometrie' published in 1779, repeated Amontons' experiments and endorsed his results that we find any further reference to the absolute scale or the zero of temperature. Lambert's observations were made with the greatest care and refinement, and resulted in correcting the value of the zero of the air scale to *minus* 270 degrees as compared with Amontons' *minus* 240 degrees. Lambert points out that the degree of temperature which is equal to zero is what one may call absolute cold, and that at this temperature the volume of the air would be practically nothing. In other words, the particles of the air would fall together and touch each other and become dense like water; and from this it may be inferred that the gaseous condition is caused by heat. Lambert says that Amontons' discoveries had found few adherents because they were too beautiful and advanced for the time in which he lived.

About this time a remarkable observation was made by Professor Braun at Moscow, who, during the severe winter of 1759, succeeded in freezing mercury by the use of a mixture of snow and nitric acid. When we remember that mercury was regarded as quite a peculiar substance possessed of the essential quality of fluidity, we can easily understand the universal interest created by the experiment of Braun. This was accentuated by the observations he made on the temperature given by the mercury thermometer, which appeared to record a temperature as low as *minus* 200° C. The experiments were soon repeated by Hutchins at Hudson's Bay, who conducted his work with the aid of suggestions given him by Cavendish and Black. The result of the new observations was to show that the freezing-point of mercury is only *minus* 40° C., the errors in former experiments having been due to the great contraction of the mercury in the thermometer in passing into the solid state. From this it followed that the enormous natural and artificial colds which had generally been believed in had no proved existence. Still the possible existence of a zero of temperature very different from that deduced from gas thermometry had the support of such distinguished names as those of Laplace and Lavoisier. In their great memoir on 'Heat,' after making what they consider reasonable hypotheses as to the relation between specific heat and total heat, they calculate values for the zero which range

from $1,500^{\circ}$ to $3,000^{\circ}$ below melting ice. On the whole, they regard the absolute zero as being in any case 600° below the freezing-point. Lavoisier, in his 'Elements of Chemistry' published in 1792, goes further in the direction of indefinitely lowering the zero of temperature when he says, 'We are still very far from being able to produce the degree of absolute cold, or total deprivation of heat, being unacquainted with any degree of coldness which we cannot suppose capable of still further augmentation; hence it follows we are incapable of causing the ultimate particles of bodies to approach each other as near as possible, and thus these particles do not touch each other in any state hitherto known.' Even as late as the beginning of the nineteenth century we find Dalton, in his new system of 'Chemical Philosophy,' giving ten calculations of this value, and adopting finally as the natural zero of temperature *minus* $3,000^{\circ}$ C.

In Black's lectures we find that he takes a very cautious view with regard to the zero of temperature, but as usual is admirably clear with regard to its exposition. Thus he says, 'We are ignorant of the lowest possible degree or beginning of heat. Some ingenious attempts have been made to estimate what it may be, but they have not proved satisfactory. Our knowledge of the degrees of heat may be compared to what we should have of a chain, the two ends of which were hidden from us and the middle only exposed to our view. We might put distinct marks on some of the links, and number the rest according as they are nearest to or further removed from the principal links; but not knowing the distance of any links from the end of the chain we could not compare them together with respect to their distance, or say that one link was twice as far from the end of the chain as another.' It is interesting to observe, however, that Black was evidently well acquainted with the work of Amontons, and strongly supports his inference as to the nature of air. Thus, in discussing the general cause of vaporisation, Black says that some philosophers have adopted the view 'that every palpable elastic fluid in nature is produced and preserved in this form by the action of heat. Mr. Amontons, an ingenious member of the late Royal Academy of Sciences, at Paris, was the first who proposed this idea with respect to the atmosphere. He supposed that it might be deprived of the whole of its elasticity and condensed and even frozen into a solid matter were it in our power to apply to it a sufficient cold; that it is a substance that differs from others by being incomparably more volatile, and which is therefore converted into vapour and preserved in that form by a weaker heat than any that ever happened or can obtain in this globe, and which therefore cannot appear under any other form than the one it now wears, so long as the constitution of the world remains the same as at present.' The views that Black attributes to Amontons have been generally associated with the name of Lavoisier, who practically admitted similar possibilities as to the nature of air; but it is not likely that in such matters Black would commit any mistake as to the real author of a particular idea, especially in his own department of knowledge. Black's own

special contribution to low-temperature studies was his explanation of the interaction of mixtures of ice with salts and acids by applying the doctrine of the latent heat of fluidity of ice to account for the frigorific effect. In a similar way Black explained the origin of the cold produced in Cullen's remarkable experiment of the evaporation of ether under the receiver of an air-pump by pointing out that the latent heat of vaporisation in this case necessitated such a result. Thus, by applying his own discoveries of latent heat, Black gave an intelligent explanation of the cause of all the low-temperature phenomena known in his day.

After the gaseous laws had been definitely formulated by Gay-Lussac and Dalton, the question of the absolute zero of temperature, as deduced from the properties of gases, was revived by Clement and Desormes. These distinguished investigators presented a paper on the subject to the French Academy in 1812, which, it appears, was rejected by that body. The authors subsequently elected to publish it in 1819. Relying on what we know now to have been a faulty hypothesis, they deduced from observations on the heating of air rushing into a vacuum the temperature of *minus* 267 degrees as that of the absolute zero. They further endeavoured to show, by extending to lower temperatures the volume or the pressure coefficients of gases given by Gay-Lussac, that at the same temperature of *minus* 267 degrees the gases would contract so as to possess no appreciable volume, or, alternatively, if the pressure was under consideration, it would become so small as to be non-existent. Although full reference is given to previous work bearing on the same subject, yet, curiously enough, no mention is made of the name of Amontons. It certainly gave remarkable support to Amontons' notion of the zero to find that simple gases like hydrogen and compound gases like ammonia, hydrochloric, carbonic, and sulphurous acids should all point to substantially the same value for this temperature. But the most curious fact about this research of Clement and Desormes is that Gay-Lussac was a bitter opponent of the validity of the inferences they drew either from his work or their own. The mode in which Gay-Lussac regarded the subject may be succinctly put as follows: A quick compression of air to one-fifth volume raises its temperature to 300 degrees, and if this could be made much greater and instantaneous the temperature might rise to 1,000 or 2,000 degrees. Conversely, if air under five atmospheres were suddenly dilated, it would absorb as much heat as it had evolved during compression, and its temperature would be lowered by 300 degrees. Therefore, if air were taken and compressed to fifty atmospheres or more, the cold produced by its sudden expansion would have no limit. In order to meet this position Clement and Desormes adopted the following reasoning: They pointed out that it had not been proved that Gay-Lussac was correct in his hypothesis, but that in any case it tacitly involves the assumption that a limited quantity of matter possesses an unlimited supply of heat. If this were the case, then heat would be unlike any other measurable thing or quality. It is, therefore, more

consistent with the course of nature to suppose that the amount of heat in a body is like the quantity of elastic fluid filling a vessel, which, while definite in original amount, one may make less and less by getting nearer to a complete exhaustion. Further, to realise the absolute zero in the one case is just as impossible as to realise the absolute vacuum in the other ; and as we do not doubt a zero of pressure, although it is unattainable, for the same reason we ought to accept the reality of the absolute zero. We know now that Gay-Lussac was wrong in supposing the increment of temperature arising from a given gaseous compression would produce a corresponding decrement from an identical expansion. After this time the zero of temperature was generally recognised as a fixed ideal point, but in order to show that it was hypothetical a distinction was drawn between the use of the expressions, zero of absolute temperature and the absolute zero.

The whole question took an entirely new form when Lord Kelvin, in 1848, after the mechanical equivalent of heat had been determined by Joule, drew attention to the great principles underlying Carnot's work on the 'Motive Power of Heat,' and applied them to an absolute method of temperature measurement, which is completely independent of the properties of any particular substance. The principle was that for a difference of one degree on this scale, between the temperatures of the source and refrigerator, a perfect engine should give the same amount of work in every part of the scale. Taking the same fixed points as for the Centigrade scale, and making 100 of the new degrees cover that range, it was found that the degrees not only within that range, but as far beyond as experimental data supplied the means of comparison, differed by only minute quantities from those of Regnault's air thermometer. The zero of the new scale had to be determined by the consideration that when the refrigerator was at the zero of temperature the perfect engine should give an amount of work equal to the full mechanical equivalent of the heat taken up. This led to a zero of 273 degrees below the temperature of freezing water, substantially the same as that deduced from a study of the gaseous state. It was a great advance to demonstrate by the application of the laws of thermodynamics not only that the zero of temperature is a reality, but that it must be located at 273 degrees below the freezing-point of water. As no one has attempted to impugn the solid foundation of theory and experiment on which Lord Kelvin based his thermodynamic scale, the existence of a definite zero of temperature must be acknowledged as a fundamental scientific fact.

Liquefaction of Gases and Continuity of State.

In these speculations, however, chemists were dealing theoretically with temperatures to which they could not make any but the most distant experimental approach. Cullen, the teacher of Black, had indeed shown how to lower temperature by the evaporation of volatile bodies,

such as ether, by the aid of the air-pump, and the later experiments of Leslie and Wollaston extended the same principle. Davy and Faraday made the most of the means at command in liquefying the more condensable gases, while at the same time Davy pointed out that they in turn might be utilised to procure greater cold by their rapid reconversion into the aeriform state. Still the chemist was sorely hampered by the want of some powerful and accessible agent for the production of temperatures much lower than had ever been attained. That want was supplied by Thilorier, who in 1835 produced liquid carbonic acid in large quantities, and further made the fortunate discovery that the liquid could be frozen into a snow by its own evaporation. Faraday was prompt to take advantage of this new and potent agent. Under exhaustion he lowered its boiling-point from *minus* 78° C. to *minus* 110° C., and by combining this low temperature with pressure all the gases were liquefied by the year 1844, with the exception of the three elementary gases—hydrogen, nitrogen, and oxygen, and three compound gases—carbonic oxide, marsh gas, and nitric oxide; Andrews some twenty-five years after the work of Faraday attempted to induce change of state in the uncondensed gases by using much higher pressures than Faraday employed. Combining the temperature of a solid carbonic acid bath with pressures of 300 atmospheres, Andrews found that none of these gases exhibited any appearance of liquefaction in such high states of condensation; but so far as change of volume by high compression went, Andrews confirmed the earlier work of Natterer by showing that the gases become proportionately less compressible with growing pressure. While such investigations were proceeding, Regnault and Magnus had completed their refined investigations on the laws of Boyle and Gay-Lussac. A very important series of experiments was made by Joule and Kelvin 'On the Thermal Effects of Fluids in Motion' about 1862, in which the thermometrical effects of passing gases under compression through porous plugs furnished important data for the study of the mutual action of the gas molecules. No one, however, had attempted to make a complete study of a liquefiable gas throughout wide ranges of temperature. This was accomplished by Andrews in 1869, and his Bakerian Lecture 'On the Continuity of the Gaseous and Liquid States of Matter' will always be regarded as an epoch-making investigation. During the course of this research Andrews observed that liquid carbonic acid raised to a temperature of 31° C. lost the sharp concave surface of demarcation between the liquid and the gas, the space being now occupied by a homogeneous fluid which exhibited, when the pressure was suddenly diminished or the temperature slightly lowered, a peculiar appearance of moving or flickering striæ, due to great local alterations of density. At temperatures above 31° C. the separation into two distinct kinds of matter could not be effected even when the pressure reached 400 atmospheres. This limiting temperature of the change of state from gas to liquid Andrews called the critical temperature. He showed that this temperature is constant, and differs with each

substance, and that it is always associated with a definite pressure peculiar to each body. Thus the two constants, critical temperature and pressure, which have been of the greatest importance in subsequent investigations, came to be defined, and a complete experimental proof was given that 'the gaseous and liquid states are only distinct stages of the same condition of matter and are capable of passing into one another by a process of continuous change.'

In 1873 an essay 'On the Continuity of the Gaseous and Liquid State,' full of new and suggestive ideas, was published by van der Waals, who, recognising the value of Clausius' new conception of the Virial in Dynamics, for a long-continued series of motions, either oscillatory or changing exceedingly slowly with time, applied it to the consideration of the molecular movements of the particles of the gaseous substance, and after much refined investigation, and the fullest experimental calculation available at the time, devised his well-known Equation of Continuity. Its paramount merit is that it is based entirely on a mechanical foundation, and is in no sense empiric; we may therefore look upon it as having a secure foundation in fact, but as being capable of extension and improvement. James Thomson, realising that the straight-line breach of continuous curvature in the Andrews isothermals was untenable to the physical mind, propounded his emendation of the Andrews curves—namely, that they were continuous and of S form. We also owe to James Thomson the conception and execution of a three-dimensional model of Andrews' results, which has been of the greatest service in exhibiting the three variables by means of a specific surface afterwards greatly extended and developed by Professor Willard Gibbs. The suggestive work of James Thomson undoubtedly was a valuable aid to van der Waals, for as soon as he reached the point where his equation had to show the continuity of the two states this was the first difficulty he had to encounter, and he succeeded in giving the explanation. He also gave a satisfactory reason for the existence of a minimum value of the product of volume and pressure in the Regnault isothermals. His isothermals, with James Thomson's completion of them, were now shown to be the results of the laws of dynamics. Andrews applied the new equation to the consideration of the coefficients of expansion with temperature and of pressure with temperature, showing that although they were nearly equal, nevertheless they were almost independent quantities. His investigation of the capillarity constant was masterly, and he added further to our knowledge of the magnitudes of the molecules of gases and of their mean free paths. Following up the experiments of Joule and Kelvin, he showed how their cooling coefficients could be deduced, and proved that they vanished at a temperature in each case which is a constant multiple of the specific critical temperature. The equation of continuity developed by van der Waals involved the use of three constants instead of one, as in the old law of Boyle and Charles, the latter being only utilised to express the relation of temperature, pressure, and volume, when the gas is far removed from its point of liquefaction. Of the two

new constants one represents the molecular pressure arising from the attraction between the molecules, the other four times the volume of the molecules. Given these constants of a gas, van der Waals showed that his equation not only fitted into the general characters of the isothermals, but also gave the values of the critical temperature, the critical pressure, and the critical volume. In the case of carbonic acid the theoretical results were found to be in remarkable agreement with the experimental values of Andrews. This gave chemists the means of ascertaining the critical constants, provided sufficiently accurate data derived from the study of a few properly distributed isothermals of the gaseous substance were available. Such important data came into the possession of chemists when Amagat published his valuable paper on 'The Isothermals of Hydrogen, Nitrogen, Oxygen, Ethylene, &c.,' in the year 1880. It now became possible to calculate the critical data with comparative accuracy for the so-called permanent gases oxygen and nitrogen, and this was done by Sarrau in 1882. In the meantime a great impulse had been given to a further attack upon the so-called permanent gases by the suggestive experiments made by Pictet and Cailletet. The static liquefaction of oxygen was effected by Wroblewski in 1883, and thereby the theoretical conclusions derived from van der Waals' equation were substantially confirmed. The liquefaction of oxygen and air was achieved through the use of liquid ethylene as a cooling agent, which enabled a temperature of *minus* 140 degrees to be maintained by its steady evaporation *in vacuo*. From this time liquid oxygen and air came to be regarded as the potential cooling agents for future research, commanding, as they did a temperature of 200 degrees below melting ice. The theoretical side of the question received at the hands of van der Waals a second contribution, which was even more important than his original essay, and that was his novel and ingenious development of what he calls 'The Theory of Corresponding States.' He defined the corresponding states of two substances as those in which the ratios of the temperature, pressure, and volume to the critical temperature, pressure, and volume respectively were the same for the two substances, and in corresponding states he showed that the three pairs of ratios all coincided. From this a series of remarkable propositions were developed, some new, some proving previous laws that were hitherto only empiric, and some completing and correcting faulty though approximate laws. As examples, he succeeded in calculating the boiling-point of carbonic acid from observations on ether vapour, proved Kopp's law of molecular volumes, and showed that at corresponding temperatures the molecular latent heats of vaporisation are proportional to the absolute critical temperature, and that under the same conditions the coefficients of liquid expansion are inversely proportional to the absolute critical temperature, and that the coefficients of liquid compressibility are inversely proportional to the critical pressure. All these propositions and deductions are in the main correct, though further experimental investigation has shown minor discrepancies requiring

explanation. Various proposals have been made to supplement van der Waals' equation so as to bring it into line with experiments, some being entirely empiric, others theoretical. Clausius, Sarrau, Wroblewski, Batteli, and others attacked the question empirically, and in the main preserved the co-volume (depending on the total volume of the molecules) unaltered while trying to modify the constant of molecular attraction. Their success depended entirely on the fact that, instead of limiting the number of constants to three, some of them have increased them to as many as ten. On the other hand, a series of very remarkable theoretical investigations has been made by van der Waals himself, by Kammerlingh Onnes, Korteweg, Jaeger, Boltzmann, Dieterici, and Rienganum, and others, all directed in the main towards an admitted variation in the value of the co-volume while preserving the molecular attraction constant. The theoretical deductions of Tait lead to the conclusion that a substance below its critical point ought to have two different equations of the van der Waals type, one referring to the liquid and the other to the gaseous phase. One important fact was soon elicited—namely, that the law of correspondence demanded only that the equation should contain not more than three constants for each body. The simplest extension is that made by Reinganum, in which he increased the pressure for a given mean kinetic energy of the particles inversely in the ratio of the diminution of free volume, due to the molecules possessing linear extension. Berthelot has shown how a 'reduced' isothermal may be got by taking two other prominent points as units of measurement instead of the critical co-ordinates. The most suggestive advance in the improvement of the van der Waals equation has been made by a lady, Mme. Christine Meyer. The idea at the base of this new development may be understood from the following general statement: van der Waals brings the van der Waals surfaces for all substances into coincidence at the point where volume, pressure, and temperature are nothing, and then stretches or compresses all the surfaces parallel to the three axes of volume, pressure, and temperature, until their critical points coincide. But on this plan the surfaces do not quite coincide, because the points where the three variables are respectively nothing are not corresponding points. Mme. Meyer's plan is to bring all the critical points first into coincidence, and then to compress or extend all the representative surfaces parallel to the three axes of volume, pressure, and temperature, until the surfaces coincide. In this way, taking twenty-nine different substances, she completely verifies from experiment van der Waals' law of correspondence. The theory of van der Waals has been one of the greatest importance in directing experimental investigation, and in attacking the difficult problems of the liquefaction of the most permanent gases. One of its greatest triumphs has been the proof that the critical constants and the boiling-point of hydrogen theoretically deduced by Wroblewski from a study of the isothermals of the gas taken far above the temperature of liquefaction are remarkably near the experimental

values. We may safely infer, therefore, that if hereafter a gas be discovered in small quantity even four times more volatile than liquid hydrogen, yet by a study of its isothermals at low temperature we shall succeed in finding its most important liquid constants, although the isolation of the real liquid may for the time be impossible. It is perhaps not too much to say that as a prolific source of knowledge in the department dealing with the continuity of state in matter, it would be necessary to go back to Carnot's cycle to find a proposition of greater importance than the theory of van der Waals and his development of the law of corresponding states.

It will be apparent from what has just been said that, thanks to the labours of Andrews, van der Waals, and others, theory had again far outrun experiment. We could calculate the constants and predict some of the simple physical characteristics of liquid oxygen, hydrogen, or nitrogen with a high degree of confidence long before any one of the three had been obtained in the static liquid condition permitting of the experimental verification of the theory. This was the more tantalising, because, with whatever confidence the chemist may anticipate the substantial corroboration of his theory, he also anticipates with almost equal conviction that as he approaches more and more nearly to the zero of absolute temperature, he will encounter phenomena compelling modification, revision, and refinement of formulas which fairly covered the facts previously known. Just as nearly seventy years ago chemists were waiting for some means of getting a temperature of 100 degrees below melting ice, so ten years ago they were casting about for the means of going 100 degrees lower still. The difficulty, it need hardly be said, increases in a geometrical rather than in an arithmetical ratio. Its magnitude may be estimated from the fact that to produce liquid air in the atmosphere of an ordinary laboratory is a feat analogous to the production of liquid water starting from steam at a white heat, and working with all the implements and surroundings at the same high temperature. The problem was not so much how to produce intense cold as how to save it when produced from being immediately levelled up by the relatively superheated surroundings. Ordinary non-conducting packings were inadmissible because they are both cumbrous and opaque, while in working near the limits of our resources it is essential that the product should be visible and readily handled. It was while puzzling over this mechanical and manipulative difficulty in 1892 that it occurred to me that the principle of an arrangement used nearly twenty years before in some calorimetric experiments, which was based upon the work of Dulong and Petit on radiation, might be employed with advantage as well to protect cold substances from heat as hot ones from rapid cooling. I therefore tried the effect of keeping liquefied gases in vessels having a double wall, the annular space between being very highly exhausted. Experiments showed that liquid air evaporated at only one-fifth of the rate prevailing when it was placed in

a similar unexhausted vessel, owing to the convective transference of heat by the gas particles being enormously reduced by the high vacuum. But, in addition, these vessels lend themselves to an arrangement by which radiant heat can also be cut off. It was found that when the inner walls were coated with a bright deposit of silver the influx of heat was diminished to one-sixth the amount entering without the metallic coating. The total effect of the high vacuum and the silvering is to reduce the ingoing heat to about 3 per cent. The efficiency of such vessels depends upon getting as high a vacuum as possible, and cold is one of the best means of effecting the desired exhaustion. All that is necessary is to fill completely the space that has to be exhausted with an easily condensable vapour, and then to freeze it out in a receptacle attached to the primary vessel that can be sealed off. The advantage of this method is that no air-pump is required, and that theoretically there is no limit to the degree of exhaustion that can be obtained. The action is rapid, provided liquid air is the cooling agent, and vapours like mercury, water, or benzol are employed. It is obvious that when we have to deal with such an exceptionally volatile liquid as hydrogen, the vapour filling may be omitted because air itself is now an easily condensable vapour. In other words, liquid hydrogen, collected in such vessels with the annular space full of air, immediately solidifies the air and thereby surrounds itself with a high vacuum. In the same way, when it shall be possible to collect a liquid boiling on the absolute scale at about 5 degrees, as compared with the 20 degrees of hydrogen, then you might have the annular space filled with the latter gas to begin with, and yet get directly a very high vacuum, owing to the solidification of the hydrogen. Many combinations of vacuum vessels can be arranged, and the lower the temperature at which we have to operate the more useful they become. Vessels of this kind are now in general use, and in them liquid air has crossed the American continent. Of the various forms, that variety is of special importance which has a spiral tube joining the bottom part of the walls, so that any liquid gas may be drawn off from the interior of such a vessel. In the working of regenerative coils such a device becomes all-important, and such special vessels cannot be dispensed with for the liquefaction of hydrogen.

In the early experiments of Pictet and Cailletet, cooling was produced by the sudden expansion of the highly compressed gas preferably at a low temperature, the former using a jet that lasted for some time, the latter an instantaneous adiabatic expansion in a strong glass tube. Neither process was practicable as a mode of producing liquid gases, but both gave valuable indications of partial change into the liquid state by the production of a temporary mist. Linde, however, saw that the continuous use of a jet of highly compressed gas, combined with regenerative cooling, must lead to liquefaction on account of what is called the Kelvin-Joule effect; and he succeeded in making a machine, based on this principle, capable of producing liquid air for industrial purposes. These experimenters had proved that, owing to molecular attraction, compressed

gases passing through a porous plug or small aperture were lowered in temperature by an amount depending on the difference of pressure and inversely as the square of the absolute temperature. This means that for a steady difference of pressure the cooling is greater the lower the temperature. The only gas that did not show cooling under such conditions was hydrogen. Instead of being cooled it became actually hotter. The reason for this apparent anomaly in the Kelvin-Joule effect is that every gas has a thermometric point of inversion above which it is heated and below which it is cooled. This inversion point, according to van der Waals, is six and three-quarter times the critical point. The efficiency of the Linde process depends on working with highly compressed gas well below the inversion temperature, and in this respect this point may be said to take the place of the critical one, when in the ordinary way direct liquefaction is being effected by the use of specific liquid cooling agents. The success of both processes depends upon working within a certain temperature range, only the Linde method gives us a much wider range of temperature within which liquefaction can be effected. This is not the case if, instead of depending on getting cooling by the internal work done by the attraction of the gas molecules, we force the compressed gas to do external work as in the well-known air machines of Kirk and Coleman. Both these inventors have pointed out that there is no limit of temperature, short of liquefaction of the gas in use in the circuit, that such machines are not capable of giving. While it is theoretically clear that such machines ought to be capable of maintaining the lowest temperatures, and that with the least expenditure of power, it is a very different matter to overcome the practical difficulties of working such machines under the conditions. Coleman kept a machine delivering air at *minus* 83 degrees for hours, but he did not carry his experiments any further. Recently Monsieur Claude, of Paris, has, however, succeeded in working a machine of this type so efficiently that he has managed to produce one litre of liquid air per horse power expended per hour in the running of the engine. This output is twice as good as that given by the Linde machine, and there is no reason to doubt that the yield will be still further improved. It is clear, therefore, that in the immediate future the production of liquid air and hydrogen will be effected most economically by the use of machines producing cold by the expenditure of mechanical work.

Liquid Hydrogen and Helium.

To the physicist the copious production of liquid air by the methods described was of peculiar interest and value as affording the means of attacking the far more difficult problem of the liquefaction of hydrogen, and even as encouraging the hope that liquid hydrogen might in time be employed for the liquefaction of yet more volatile elements, apart from the importance which its liquefaction must hold in the process of the steady advance towards the absolute zero. Hydrogen is an element of especial

interest, because the study of its properties and chemical relations led great chemists like Faraday, Dumas, Daniell, Graham, and Andrews to entertain the view that if it could ever be brought into the state of liquid or solid it would reveal metallic characters. Looking to the special chemical relations of the combined hydrogen in water, alkaline oxides, acids, and salts, together with the behaviour of these substances on electrolysis, we are forced to conclude that hydrogen behaves as the analogue of a metal. After the beautiful discovery of Graham that palladium can absorb some hundreds of times its own volume of hydrogen, and still retain its lustre and general metallic character, the impression that hydrogen was probably a member of the metallic group became very general. The only chemist who adopted another view was my distinguished predecessor, Professor Odling. In his 'Manual of Chemistry,' published in 1861, he pointed out that hydrogen has chlorous as well as basic relations, and that they are as decided, important, and frequent as its other relations. From such considerations he arrived at the conclusion that hydrogen is essentially a neutral or intermediate body, and therefore we should not expect to find liquid or solid hydrogen possess the appearance of a metal. This extraordinary prevision, so characteristic of Odling, was proved to be correct some thirty-seven years after it was made. Another curious anticipation was made by Dumas in a letter addressed to Pictet, in which he says that the metal most analogous to hydrogen is magnesium, and that probably both elements have the same atomic volume, so that the density of hydrogen, for this reason, would be about the value elicited by subsequent experiments. Later on, in 1872, when Newlands began to arrange the elements in periodic groups, he regarded hydrogen as the lowest member of the chlorine family; but Mendeleef in his later classification placed hydrogen in the group of the alkaline metals; on the other hand, Dr. Johnstone Stoney classes hydrogen with the alkaline earth metals and magnesium. From this speculative divergency it is clear no definite conclusion could be reached regarding the physical properties of liquid or solid hydrogen, and the only way to arrive at the truth was to prosecute low-temperature research until success attended the efforts to produce its liquefaction. This result I definitively obtained in 1898. The case of liquid hydrogen is, in fact, an excellent illustration of the truth already referred to, that no theoretical forecast, however apparently justified by analogy, can be finally accepted as true until confirmed by actual experiment. Liquid hydrogen is a colourless transparent body of extraordinary intrinsic interest. It has a clearly defined surface, is easily seen, drops well, in spite of the fact that its surface tension is only the thirty-fifth part of that of water, or about one-fifth that of liquid air, and can be poured easily from vessel to vessel. The liquid does not conduct electricity, and, if anything, is slightly diamagnetic. Compared with an equal volume of liquid air, it requires only one-fifth the quantity of heat for vaporisation; on the other hand, its specific heat is ten times that of liquid air or five times

that of water. The coefficient of expansion of the fluid is remarkable, being about ten times that of gas; it is by far the lightest liquid known to exist, its density being only one-fourteenth that of water; the lightest liquid previously known was liquid marsh gas, which is six times heavier. The only solid which has so small density as to float upon its surface is a piece of pith wood. It is by far the coldest liquid known. At ordinary atmospheric pressure it boils at *minus* 252·5 degrees or 20·5 degrees absolute. The critical point of the liquid is about 29 degrees absolute, and the critical pressure not more than fifteen atmospheres. The vapour of the hydrogen arising from the liquid has nearly the density of air—that is, it is fourteen times that of the gas at the ordinary temperature. Reduction of the pressure by an air-pump brings down the temperature to *minus* 258 degrees, when the liquid becomes a solid resembling frozen foam, and this by further exhaustion is cooled to *minus* 260 degrees, or 13 degrees absolute, which is the lowest steady temperature that has been reached. The solid may also be got in the form of a clear transparent ice, melting at about 15 degrees absolute, under a pressure of 55 mm., possessing the unique density of one-eleventh that of water. Such cold involves the solidification of every gaseous substance but one that is at present definitely known to the chemist, and so liquid hydrogen introduces the investigator to a world of solid bodies. The contrast between this refrigerating substance and liquid air is most remarkable. On the removal of the loose plug of cotton-wool used to cover the mouth of the vacuum vessel in which it is stored, the action is followed by a miniature snowstorm of solid air, formed by the freezing of the atmosphere at the point where it comes into contact with the cold vapour rising from the liquid. This solid air falls into the vessel and accumulates as a white snow at the bottom of the liquid hydrogen. When the outside of an ordinary test-tube is cooled by immersion in the liquid, it is soon observed to fill up with solid air, and if the tube be now lifted out a double effect is visible, for liquid air is produced both in the inside and on the outside of the tube—in the one case by the melting of the solid, and in the other by condensation from the atmosphere. A tuft of cotton-wool soaked in the liquid and then held near the pole of a strong magnet is attracted, and it might be inferred therefrom that liquid hydrogen is a magnetic body. This, however, is not the case: the attraction is due neither to the cotton-wool nor to the hydrogen—which indeed evaporates almost as soon as the tuft is taken out of the liquid—but to the oxygen of the air, which is well known to be a magnetic body, frozen in the wool by the extreme cold.

The strong condensing powers of liquid hydrogen afford a simple means of producing vacua of very high tenuity. When one end of a sealed tube containing ordinary air is placed for a short time in the liquid, the contained air accumulates as a solid at the bottom, while the higher part is almost entirely deprived of particles of gas. So perfect is the vacuum thus formed, that the electric discharge can be made to pass only

with the greatest difficulty. Another important application of liquid air, liquid hydrogen, &c., is as analytic agents. Thus, if a gaseous mixture be cooled by means of liquid oxygen, only those constituents will be left in the gaseous state which are less condensable than oxygen. Similarly, if this gaseous residue be in its turn cooled in liquid hydrogen a still further separation will be effected, everything that is less volatile than hydrogen being condensed to a liquid or solid. By proceeding in this fashion it has been found possible to isolate helium from a mixture in which it is present to the extent of only one part in one thousand. By the evaporation of solid hydrogen under the air-pump we can reach within 13 or 14 degrees of the zero, but there or thereabouts our progress is barred. This gap of 13 degrees might seem at first sight insignificant in comparison with the hundreds that have already been conquered. But to win one degree low down the scale is quite a different matter from doing so at higher temperatures; in fact, to annihilate these few remaining degrees would be a far greater achievement than any so far accomplished in low-temperature research. For the difficulty is twofold, having to do partly with process and partly with material. The application of the methods used in the liquefaction of gases becomes continually harder and more troublesome as the working temperature is reduced; thus, to pass from liquid air to liquid hydrogen—a difference of 60 degrees—is, from a thermodynamic point of view, as difficult as to bridge the gap of 150 degrees that separates liquid chlorine and liquid air. By the use of a new liquid gas exceeding hydrogen in volatility to the same extent as hydrogen does nitrogen, the investigator might get to within five degrees of the zero; but even a second hypothetical substance, again exceeding the first one in volatility to an equal extent, would not suffice to bring him quite to the point of his ambition. That the zero will ever be reached by man is extremely improbable. A thermometer introduced into regions outside the uttermost confines of the earth's atmosphere might approach the absolute zero, provided that its parts were highly transparent to all kinds of radiation, otherwise it would be affected by the radiation of the sun, and would therefore become heated. But supposing all difficulties to be overcome, and the experimenter to be able to reach within a few degrees of the zero, it is by no means certain that he would find the near approach of the death of matter sometimes pictured. Any forecast of the phenomena that would be seen must be based on the assumption that there is continuity between the processes studied at attainable temperatures and those which take place at still lower ones. Is such an assumption justified? It is true that many changes in the properties of substances have been found to vary steadily with the degree of cold to which they are exposed. But it would be rash to take for granted that the changes which have been traced in explored regions continue to the same extent and in the same direction in those which are as yet unexplored. Of such a breakdown low-temperature research has already

yielded a direct proof at least in one case. A series of experiments with pure metals showed that their electrical resistance gradually decreases as they are cooled to lower and lower temperatures, in such ratio that it appeared probable that at the zero of absolute temperature they would have no resistance at all and would become perfect conductors of electricity. This was the inference that seemed justifiable by observations taken at depths of cold which can be obtained by means of liquid air and less powerful refrigerants. But with the advent of the more powerful refrigerant liquid hydrogen it became necessary to revise that conclusion. A discrepancy was first observed when a platinum resistance thermometer was used to ascertain the temperature of that liquid boiling under atmospheric and reduced pressure. All known liquids, when forced to evaporate quickly by being placed in the exhausted receiver of an air-pump, undergo a reduction in temperature, but when hydrogen was treated in this way it appeared to be an exception. The resistance thermometer showed no such reduction as was expected, and it became a question whether it was the hydrogen or the thermometer that was behaving abnormally. Ultimately, by the adoption of other thermometrical appliances, the temperature of the hydrogen was proved to be lowered by exhaustion as theory indicated. Hence it was the platinum thermometer which had broken down; in other words, the electrical resistance of the metal employed in its construction was not, at temperatures about *minus* 250° C., decreased by cold in the same proportion as at temperatures about *minus* 200°. This being the case, there is no longer any reason to suppose that at the absolute zero platinum would become a perfect conductor of electricity; and in view of the similarity between the behaviour of platinum and that of other pure metals in respect of temperature and conductivity, the presumption is that the same is true of them also. At any rate, the knowledge that in the case of at least one property of matter we have succeeded in attaining a depth of cold sufficient to bring about unexpected change in the law expressing the variation of that property with temperature, is sufficient to show the necessity for extreme caution in extending our inferences regarding the properties of matter near the zero of temperature. Lord Kelvin evidently anticipates the possibility of more remarkable electrical properties being met with in the metals near the zero. A theoretical investigation on the relation of 'electrions' and atoms has led him to suggest a hypothetical metal having the following remarkable properties: below 1 degree absolute it is a perfect insulator of electricity, at 2 degrees it shows noticeable conductivity, and at 6 degrees it possesses high conductivity. It may safely be predicted that liquid hydrogen will be the means by which many obscure problems of physics and chemistry will ultimately be solved, so that the liquefaction of the last of the old permanent gases is as pregnant now with future consequences of great scientific moment as was the liquefaction of chlorine in the early years of the last century.

The next step towards the absolute zero is to find another gas more volatile than hydrogen, and that we possess in the gas occurring in cleveite,

identified by Ramsay as helium, a gas which is widely distributed, like hydrogen, in the sun, stars, and nebulae. A specimen of this gas was subjected by Olszewski to liquid air temperatures, combined with compression and subsequent expansion, following the Cailletet method, and resulted in his being unable to discover any appearance of liquefaction, even in the form of mist. His experiments led him to infer that the boiling-point of the substance is probably below 9 degrees absolute. After Lord Rayleigh had found a new source of helium in the gases which are derived from the Bath springs, and liquid hydrogen became available as a cooling agent, a specimen of helium cooled in liquid hydrogen showed the formation of fluid, but this turned out to be owing to the presence of an unknown admixture of other gases. As a matter of fact, a year before the date of this experiment I had recorded indications of the presence of unknown gases in the spectrum of helium derived from this source. When subsequently such condensable constituents were removed, the purified helium showed no signs of liquefaction, even when compressed to 80 atmospheres, while the tube containing it was surrounded with solid hydrogen. Further, on suddenly expanding, no instantaneous mist appeared. Thus helium was definitely proved to be a much more volatile substance than hydrogen in either the liquid or solid condition. The inference to be drawn from the adiabatic expansion effected under the circumstances is that helium must have touched a temperature of from 9 to 10 degrees for a short time without showing any signs of liquefaction, and consequently that the critical point must be still lower. This would force us to anticipate that the boiling-point of the liquid will be about 5 degrees absolute, or liquid helium will be four times more volatile than liquid hydrogen, just as liquid hydrogen is four times more volatile than liquid air. Although the liquefaction of the gas is a problem for the future, this does not prevent us from safely anticipating some of the properties of the fluid body. It would be twice as dense as liquid hydrogen, with a critical pressure of only 4 or 5 atmospheres. The liquid would possess a very feeble surface-tension, and its compressibility and expansibility would be about four times that of liquid hydrogen, while the heat required to vaporise the molecule would be about one-fourth that of liquid hydrogen. Heating the liquid 1 degree above its boiling-point would raise the pressure by $1\frac{3}{4}$ atmospheres, which is more than four times the increment for liquid hydrogen. The liquid would be only seventeen times denser than its vapour, whereas liquid hydrogen is sixty-five times denser than the gas it gives off. Only some 3 or 4 degrees would separate the critical temperature from the boiling-point and the melting-point, whereas in liquid hydrogen the separation is respectively 10 and 15 degrees. As the liquid refractivities for oxygen, nitrogen, and hydrogen are closely proportional to the gaseous values, and as Lord Rayleigh has shown that helium has only one-fourth the refractivity of hydrogen, although it is twice as dense, we must infer that the refractivity of liquid helium would also be about one-

fourth that of liquid hydrogen. Now hydrogen has the smallest refractivity of any known liquid, and yet liquid helium will have only about one-fourth of this value—comparable, in fact, with liquid hydrogen just below its critical point. This means that the liquid will be quite exceptional in its optical properties, and very difficult to see. This may be the explanation of why no mist has been seen on its adiabatic expansion from the lowest temperatures. Taking all these remarkable properties of the liquid into consideration, one is afraid to predict that we are at present able to cope with the difficulties involved in its production and collection. Provided the critical point is, however, not below 8 degrees absolute, then from the knowledge of the conditions that are successful in producing a change of state in hydrogen through the use of liquid air, we may safely predict that helium can be liquefied by following similar methods. If, however, the critical point is as low as 6 degrees absolute, then it would be almost hopeless to anticipate success by adopting the process that works so well with hydrogen. The present anticipation is that the gas will succumb after being subjected to this process, only, instead of liquid air under exhaustion being used as the primary cooling agent, liquid hydrogen evaporating under similar circumstances must be employed. In this case the resulting liquid would require to be collected in a vacuum vessel, the outer walls of which are immersed in liquid hydrogen. The practical difficulties and the cost of the operation will be very great; but on the other hand, the descent to a temperature within 5 degrees of the zero would open out new vistas of scientific inquiry, which would add immensely to our knowledge of the properties of matter. To command in our laboratories a temperature which would be equivalent to that which a comet might reach at an infinite distance from the sun would indeed be a great triumph for science. If the present Royal Institution attack on helium should fail, then we must ultimately succeed by adopting a process based on the mechanical production of cold through the performance of external work. When a turbine can be worked by compressed helium, the whole of the mechanism and circuits being kept surrounded with liquid hydrogen, then we need hardly doubt that the liquefaction will be effected. In all probability gases other than helium will be discovered of greater volatility than hydrogen. It was at the British Association Meeting in 1896 that I made the first suggestion of the probable existence of an unknown element which would be found to fill up the gap between argon and helium, and this anticipation was soon taken up by others and ultimately confirmed. Later, in the Bakerian Lecture for 1901, I was led to infer that another member of the helium group might exist having the atomic weight about 2, and this would give us a gas still more volatile, with which the absolute zero might be still more nearly approached. It is to be hoped that some such element or elements may yet be isolated and identified as coronium or nebulium. If amongst the unknown gases possessing a very low critical point some have a high critical pressure, instead of a low one, which ordinary experience would lead us to antici-

pate, then such difficultly liquefiable gases would produce fluids having different physical properties from any of those with which we are acquainted. Again, gases may exist having smaller atomic weights and densities than hydrogen, yet all such gases must, according to our present views of the gaseous state, be capable of liquefaction before the zero of temperature is reached. The chemists of the future will find ample scope for investigation within the apparently limited range of temperature which separates solid hydrogen from the zero. Indeed, great as is the sentimental interest attached to the liquefaction of these refractory gases, the importance of the achievement lies rather in the fact that it opens out new fields of research and enormously widens the horizon of physical science, enabling the natural philosopher to study the properties and behaviour of matter under entirely novel conditions. This department of inquiry is as yet only in its infancy, but speedy and extensive developments may be looked for, since within recent years several special cryogenic laboratories have been established for the prosecution of such researches, and a liquid-air plant is becoming a common adjunct to the equipment of the ordinary laboratory.

The Upper Air and Auroras.

The present liquid ocean, neglecting everything for the moment but the water, was at a previous period of the earth's history part of the atmosphere, and its condensation has been brought about by the gradual cooling of the earth's surface. This resulting ocean is subjected to the pressure of the remaining uncondensed gases, and as these are slightly soluble they dissolve to some extent in the fluid. The gases in solution can be taken out by distillation or by exhausting the water, and if we compare their volume with the volume of the water as steam, we should find about 1 volume of air in 60,000 volumes of steam. This would then be about the rough proportion of the relatively permanent gas to condensable gas which existed in the case of the vaporised ocean. Now let us assume the surface of the earth gradually cooled to some 200 degrees below the freezing-point; then, after all the present ocean was frozen, and the climate became three times more intense than any arctic frost, a new ocean of liquid air would appear, covering the entire surface of the frozen globe about thirty-five feet deep. We may now apply the same reasoning to the liquid air ocean that we formerly did to the water one, and this would lead us to anticipate that it might contain in solution some gases that may be far less condensable than the chief constituents of the fluid. In order to separate them we must imitate the method of taking the gases out of water. Assume a sample of liquid air cooled to the low temperature that can be reached by its own evaporation, connected by a pipe to a condenser cooled in liquid hydrogen; then any volatile gases present in solution will distil over with the first portions of the air, and can be pumped off, being uncondensable

at the temperature of the condenser. In this way, a gas mixture, containing, of the known gases, free hydrogen, helium, and neon, has been separated from liquid air. It is interesting to note in passing that the relative volatilities of water and oxygen are in the same ratio as those of liquid air and hydrogen, so that the analogy between the ocean of water and that of liquid air has another suggestive parallel. The total uncondensable gas separated in this way amounts to about one fifty-thousandth of the volume of the air, which is about the same proportion as the air dissolved in water. That free hydrogen exists in air in small amount is conclusively proved, but the actual proportion found by the process is very much smaller than Gautier has estimated by the combustion method. The recent experiments of Lord Rayleigh show that Gautier, who estimated the hydrogen present as one five-thousandth, has in some way produced more hydrogen than he can manage to extract from pure air by a repetition of the same process. The spectroscopic examination of these gases throws new light upon the question of the aurora and the nature of the upper air. On passing electric discharges through the tubes containing the most volatile of the atmospheric gases, they glow with a bright orange light, which is especially marked at the negative pole. The spectroscope shows that this light consists, in the visible part of the spectrum, chiefly of a succession of strong rays in the red, orange, and yellow, attributed to hydrogen, helium, and neon. Besides these, a vast number of rays, generally less brilliant, are distributed through the whole length of the visible spectrum. The greater part of these rays are of, as yet, unknown origin. The violet and ultra-violet part of the spectrum rivals in strength that of the red and yellow rays. As these gases probably include some of the gases that pervade interplanetary space, search was made for the prominent nebular, coronal, and auroral lines. No definite lines agreeing with the nebular spectrum could be found, but many lines occurred closely coincident with the coronal and auroral spectrum. But before discussing the spectroscopic problem it will be necessary to consider the nature and condition of the upper air.

According to the old law of Dalton, supported by the modern dynamical theory of gases, each constituent of the atmosphere while acted upon by the force of gravity forms a separate atmosphere, completely independent, except as to temperature, of the others, and the relations between the common temperature and the pressure and altitude for each specific atmosphere can be definitely expressed. If we assume the altitude and temperature known, then the pressure can be ascertained for the same height in the case of each of the gaseous constituents, and in this way the percentage composition of the atmosphere at that place may be deduced. Suppose we start with a surface atmosphere having the composition of our air, only containing two ten-thousandths of hydrogen, then at thirty-seven miles, if a sample could be procured for analysis, we believe that it would be found to contain 12 per cent. of hydrogen and

only 10 per cent. of oxygen. The carbonic acid practically disappears ; and by the time we reach forty-seven miles, where the temperature is *minus* 132 degrees, assuming a gradient of 3.2 degrees per mile, the nitrogen and oxygen have so thinned out that the only constituent of the upper air which is left is hydrogen. If the gradient of temperature were doubled, the elimination of the nitrogen and oxygen would take place by the time thirty-seven miles was reached, with a temperature of *minus* 220 degrees. The permanence of the composition of the air at the highest altitudes, as deduced from the basis of the dynamical theory of gases, has been discussed by Stoney, Bryan, and others. It would appear that there is a consensus of opinion that the rate at which gases like hydrogen and helium could escape from the earth's atmosphere would be excessively slow. Considering that to compensate any such loss the same gases are being supplied by actions taking place in the crust of the earth, we may safely regard them as necessarily permanent constituents of the upper air. The temperature at the elevations we have been discussing would not be sufficient to cause any liquefaction of the nitrogen and oxygen, the pressure being so low. If we assume the mean temperature as about the boiling-point of oxygen at atmospheric pressure, then a considerable amount of the carbonic acid must solidify as a mist, if the air from a lower level be cooled to this temperature ; and the same result might take place with other gases of relatively small volatility which occur in air. This would explain the clouds that have been seen at an elevation of fifty miles, without assuming the possibility of water vapour being carried up so high. The temperature of the upper air must be above that on the vapour pressure curve corresponding to the barometric pressure at the locality, otherwise liquid condensation must take place. In other words, the temperature must be above the dew-point of air at that place. At higher elevations, on any reasonable assumption of temperature distribution, we inevitably reach a temperature where the air would condense, just as Fourier and Poisson supposed it would, unless the temperature is arrested in some way from approaching the zero. Both ultra-violet absorption and the prevalence of electric storms may have something to do with the maintenance of a higher mean temperature. The whole mass of the air above forty miles is not more than one seven-hundredth part of the total mass of the atmosphere, so that any rain or snow of liquid or solid air, if it did occur, would necessarily be of a very tenuous description. In any case, the dense gases tend to accumulate in the lower strata, and the lighter ones to predominate at the higher altitudes, always assuming that a steady state of equilibrium has been reached. It must be observed, however, that a sample of air taken at an elevation of nine miles has shown no difference in composition from that at the ground, whereas, according to our hypothesis, the oxygen ought to have been diminished to 17 per cent., and the carbonic acid should also have become much less. This can only be explained by assuming that a large intermixture of different layers of the atmosphere is still taking place at this.

elevation. This is confirmed by a study of the motions of clouds about six miles high, which reveals an average velocity of the air currents of some seventy miles an hour ; such violent winds must be the means of causing the intermingling of different atmospheric strata. Some clouds, however, during hot and thundery weather, have been seen to reach an elevation of seventeen miles, so that we have direct proof that on occasion the lower layers of atmosphere are carried to a great elevation. The existence of an atmosphere at more than a hundred miles above the surface of the earth is revealed to us by the appearance of meteors and fireballs, and when we can take photographs of the spectrum of such apparitions we shall learn a great deal about the composition of the upper air. In the meantime Pickering's solitary spectrum of a meteor reveals an atmosphere of hydrogen and helium, and so far this is corroborative of the doctrine we have been discussing. It has long been recognised that the aurora is the result of electric discharges within the limits of the earth's atmosphere, but it was difficult to understand why its spectrum should be so entirely different from anything which could be produced artificially by electric discharges through rarefied air at the surface of the earth. Writing in 1879, Rand Capron, after collecting all the recorded observations, was able to enumerate no more than nine auroral rays, of which but one could with any probability be identified with rays emitted by atmospheric air under an electric discharge. Vogel attributed this want of agreement between nature and experiment, in a vague way, to difference of temperature and pressure ; and Zollner thought the auroral spectrum to be one of a different order, in the sense in which the line and band spectra of nitrogen are said to be of different orders. Such statements were merely confessions of ignorance. But since that time observations of the spectra of auroras have been greatly multiplied, chiefly through the Swedish and Danish Polar Expeditions, and the length of spectrum recorded on the ultra-violet side has been greatly extended by the use of photography, so that, in a recent discussion of the results, M. Henri Stassano is able to enumerate upwards of one hundred auroral rays, of which the wave-length is more or less approximately known, some of them far in the ultra-violet. Of this large number of rays he is able to identify, within the probable limits of errors of observation, about two-thirds as rays, which Professor Liveing and myself have observed to be emitted by the most volatile gases of atmospheric air unliquefiable at the temperature of liquid hydrogen. Most of the remainder he ascribes to argon, and some he might, with more probability, have identified with krypton or xenon rays, if he had been aware of the publication of wave-lengths of the spectra of those gases, and the identification of one of the highest rays of krypton with that most characteristic of auroras. The rosy tint often seen in auroras, particularly in the streamers, appears to be due mainly to neon, of which the spectrum is remarkably rich in red and orange rays. One or two neon rays are amongst those most frequently observed, while the red ray of hydrogen and one red ray of krypton have been noticed only

once. The predominance of neon is not surprising, seeing that from its relatively greater proportion in air and its low density it must tend to concentrate at higher elevations. So large a number of probable identifications warrants the belief that we may yet be able to reproduce in our laboratories the auroral spectrum in its entirety. It is true that we have still to account for the appearance of some, and the absence of other, rays of the newly discovered gases, which in the way in which we stimulate them appear to be equally brilliant, and for the absence, with one doubtful exception, of all the rays of nitrogen. If we cannot give the reason of this, it is because we do not know the mechanism of luminescence—nor even whether the particles which carry the electricity are themselves luminous, or whether they only produce stresses causing other particles which encounter them to vibrate; yet we are certain that an electric discharge in a highly rarefied mixture of gases lights one element and not another, in a way which, to our ignorance, seems capricious. The Swedish North Polar Expedition concluded from a great number of trigonometrical measurements that the average above the ground of the base of the aurora was fifty kilometres (thirty-four miles) at Cape Thorsden, Spitzbergen; at this height the pressure of the nitrogen of the atmosphere would be only about one-tenth of a millimetre, and Moissan and Deslandres have found that in atmospheric air at pressures less than one millimetre the rays of nitrogen and oxygen fade and are replaced by those of argon and by five new rays which Stassano identifies with rays of the more volatile gases measured by us. Also Collie and Ramsay's observations on the distance to which electrical discharges of equal potential traverse different gases explosively throw much light on the question; for they find that, while for helium and neon this distance is from 250 to 300 mm., for argon it is $45\frac{1}{2}$ mm., for hydrogen it is 39 mm., and for air and oxygen still less. This indicates that a good deal depends on the very constitution of the gases themselves, and certainly helps us to understand why neon and argon, which exist in the atmosphere in larger proportions than helium, krypton, or xenon, should make their appearance in the spectrum of auroras almost to the exclusion of nitrogen and oxygen. How much depends not only on the constitution and it may be temperature of the gases, but also on the character of the electric discharge, is evident from the difference between the spectra at the cathode and anode in different gases, notably in nitrogen and argon, and not less remarkably in the more volatile compounds of the atmosphere. Paulsen thinks the auroral spectrum wholly due to cathodic rays. Without stopping to discuss that question, it is certain that changes in the character of the electric discharge produce definite changes in the spectra excited by them. It has long been known that in many spectra the rays which are inconspicuous with an uncondensed electric discharge become very pronounced when a Leyden jar is in the circuit. This used to be ascribed to a higher temperature in this condensed spark, though measurements of that

temperature have not borne out the explanation. Schuster and Hemsalech have shown that these changes of spectra are in part due to the oscillatory character of the condenser discharge which may be enhanced by self-induction, and the corresponding change of spectrum thereby made more pronounced. Lightning we should expect to resemble condensed discharge much more than aurora, but this is not borne out by the spectrum. Pickering's recent analysis of the spectrum of a flash obtained by photography shows, out of nineteen lines measured by him, only two which can be assigned with probability to nitrogen and oxygen, while three hydrogen rays most likely due to water are very conspicuous, and eleven may be reasonably ascribed to argon, krypton, and xenon, one to more volatile gas of the neon class, and the brightest ray of all is but a very little less refrangible than the characteristic auroral ray, and coincides with a strong ray of calcium, but also lies between, and close to, an argon and a neon ray, neither of them weak rays. There may be some doubt about the identification of the spectral rays of auroras because of the wide limits of the probable errors in measuring wave-lengths so faint as most of them are, but there is no such doubt about the wave-lengths of the rays in solar protuberances measured by Deslandres and Hale. Stassano found that these rays, forty-four in number, lying between the Fraunhofer line H' and 3148 in the ultra-violet agree very closely with rays which Professor Liveing and myself measured in the spectra of the most volatile atmospheric gases. It will be remembered that one of the earliest suggestions as to the nature of solar prominences was that they were solar auroras. This supposition helped to explain the marvellous rapidity of their changes, and the apparent suspension of brilliant self-luminous clouds at enormous heights above the sun's surface. Now the identification of the rays of their spectra with those of the most volatile gases, which also furnish many of the auroral rays, certainly supports that suggestion. A stronger support, however, seems to be given to it by the results obtained at the total eclipse of May 1901, by the American expedition to Sumatra. In the 'Astrophysical Journal' for June last is a list of 339 lines in the spectrum of the corona photographed by Humphreys, during totality, with a very large concave grating. Of these no fewer than 209 do not differ from lines we have measured in the most volatile gases of the atmosphere, or in krypton or xenon, by more than one unit of wave-length on Armstrong's scale, a quantity within the limit of probable error. Of the remainder, a good many agree to a like degree with argon lines, a very few with oxygen lines, and still fewer with nitrogen lines; the characteristic green auroral ray, which is not in the range of Humphreys' photographs, also agrees within a small fraction of a unit of wave-length with one of the rays emitted by the most volatile atmospheric gas. Taking into account the Fraunhofer lines H , K , and G , usually ascribed to calcium, there remain only fifty-five lines of the 339 unaccounted for to the degree of probability indicated. Of these considerably more than half are very weak lines which have not depicted

themselves on more than one of the six films exposed, and extend but a very short distance into the sun's atmosphere. There are, however, seven which are stronger lines, and reach to a considerable height above the sun's rim, and all have depicted themselves on at least four of the six films. If there be no considerable error in the wave-lengths assigned (and such is not likely to be the case), these lines may perhaps be due to some volatile element which may yet be discovered in our atmosphere. However that may be, the very great number of close coincidences between the auroral rays and those which are emitted under electric excitement by gases of our atmosphere almost constrains us to believe, what is indeed most probable on other grounds, that the sun's coronal atmosphere is composed of the same substances as the earth's, and that it is rendered luminous in the same way—namely, by electric discharges. This conclusion has plainly an important bearing on the explanation which should be given of the outburst of new stars and of the extraordinary and rapid changes in their spectra. Moreover, leaving on one side the question whether gases ever become luminous by the direct action of heat, apart from such transfers of energy as occur in chemical change and electric disturbance, it demands a revision of the theories which attribute more permanent differences between the spectra of different stars to differences of temperature, and a fuller consideration of the question whether they cannot with better reason be explained by differences in the electric conditions which prevail in the stellar atmosphere.

If we turn to the question what is the cause of the electric discharges which are generally believed to occasion auroras, but of which little more has hitherto been known than that they are connected with sun-spots and solar eruptions, recent studies of electric discharges in high vacua, with which the names of Crookes, Röntgen, Lenard, and J. J. Thomson will always be associated, have opened the way for Arrhenius to suggest a definite and rational answer. He points out that the frequent disturbances which we know to occur in the sun must cause electric discharges in the sun's atmosphere far exceeding any that occur in that of the earth. These will be attended with an ionisation of the gases, and the negative ions will stream away through the outer atmosphere of the sun into the interplanetary space, becoming, as Wilson has shown, nuclei of aggregation of condensable vapours and cosmic dust. The liquid and solid particles thus formed will be of various sizes; the larger will gravitate back to the sun, while those with diameters less than one and a half thousandths of a millimetre, but nevertheless greater than a wave-length of light, will, in accordance with Clerk-Maxwell's electromagnetic theory, be driven away from the sun by the incidence of the solar rays upon them, with velocities which may become enormous, until they meet other celestial bodies, or increase their dimensions by picking up more cosmic dust or diminish them by evaporation. The earth will catch its share of such particles on the side which is turned towards the sun, and its upper atmosphere will thereby become negatively electrified until the

potential of the charge reaches such a point that a discharge occurs, which will be repeated as more charged particles reach the earth. This theory not only accounts for the auroral discharges, and the coincidence of their times of greatest frequency with those of the maxima of sunspots, but also for the minor maxima and minima. The vernal and autumnal maxima occur when the line through the earth and sun has its greatest inclination to the solar equator, so that the earth is more directly exposed to the region of maximum of sunspots, while the twenty-six days period corresponds closely with the period of rotation of that part of the solar surface where faculae are most abundant. J. J. Thomson has pointed out, as a consequence of the Richardson observations, that negative ions will be constantly streaming from the sun merely regarded as a hot body, but this is not inconsistent with the supposition that there will be an excess of this emission in eruptions, and from the regions of faculae. Arrhenius' theory accounts also, in a way which seems the most satisfactory hitherto enunciated, for the appearances presented by comets. The solid parts of these objects absorb the sun's rays, and as they approach the sun become heated on the side turned towards him until the volatile substances frozen in or upon them are evaporated and diffused in the gaseous state in surrounding space, where they get cooled to the temperature of liquefaction and aggregated in drops about the negative ions. The larger of these drops gravitate towards the sun and form clouds of the coma about the head, while the smaller are driven by the incidence of the sun's light upon them away from the sun and form the tail. The curvature of the tail depends, as Bredichin has shown, on the rate at which the particles are driven, which in turn depends on the size and specific gravity of the particles, and these will vary with the density of the vapour from which they are formed and the frequency of the negative ions which collect them. In any case Arrhenius' theory is a most suggestive one, not only with reference to auroras and comets, and the solar corona and chromosphere, but also as to the constitution of the photosphere itself.

Various Low-Temperature Researches.

We may now summarise some of the results which have already been attained by low-temperature studies. In the first place, the great majority of chemical interactions are entirely suspended, but an element of such exceptional powers of combination as fluorine is still active at the temperature of liquid air. Whether solid fluorine and liquid hydrogen would interact no one can at present say. Bodies naturally become denser, but even a highly expansive substance like ice does not appear to reach the density of water at the lowest temperature. This is confirmatory of the view that the particles of matter under such conditions are not packed in the closest possible way. The force of cohesion is greatly increased at low temperatures, as is shown by the additional stress required to rupture metallic wires. This fact is of interest in connection with two conflicting theories of matter. Lord Kelvin's view is that the forces that hold

together the particles of bodies may be accounted for without assuming any other agency than gravitation or any other law than the Newtonian. An opposite view is that the phenomena of the aggregation of molecules depend upon the molecular vibration as a physical cause. Hence, at the zero of absolute temperature, this vibrating energy being in complete abeyance, the phenomena of cohesion should cease to exist, and matter generally be reduced to an incoherent heap of cosmic dust. This second view receives no support from experiment.

The photographic action of light is diminished at the temperature of liquid air to about 20 per cent. of its ordinary efficiency, and at the still lower temperature of liquid hydrogen only about 10 per cent. of the original sensitivity remains. At the temperature of liquid air or liquid hydrogen a large range of organic bodies and many inorganic ones acquire under exposure to violet light the property of phosphorescence. Such bodies glow faintly so long as they are kept cold, but become exceedingly brilliant during the period when the temperature is rising. Even solid air is a phosphorescent body. All the alkaline earth sulphides which phosphoresce brilliantly at the ordinary temperature lose this property when cooled, to be revived on heating; but such bodies in the first instance may be stimulated through the absorption of light at the lowest temperatures. Radio-active bodies, on the other hand, like radium, which are naturally self-luminous, maintain this luminosity unimpaired at the very lowest temperatures, and are still capable of inducing phosphorescence in bodies like the platino-cyanides. Some crystals become for a time self-luminous when cooled in liquid air or hydrogen, owing to the induced electric stimulation causing discharges between the crystal molecules. This phenomenon is very pronounced with nitrate of uranium and some platino-cyanides.

In conjunction with Professor Fleming a long series of experiments was made on the electric and magnetic properties of bodies at low temperatures. The subjects that have been under investigation may be classified as follows: The Thermo-Electric Powers of Pure Metals; The Magnetic Properties of Iron and Steel; Dielectric Constants; The Magnetic and Electric Constants of Liquid Oxygen; Magnetic Susceptibility.

The investigations have shown that electric conductivity in pure metals varies almost inversely as the absolute temperature down to *minus* 200 degrees, but that this law is greatly affected by the presence of the most minute amount of impurity. Hence the results amount to a proof that electric resistance in pure metals is closely dependent upon the molecular or atomic motion which gives rise to temperature, and that the process by which the energy constituting what is called an electric current is dissipated essentially depends upon non-homogeneity of structure and upon the absolute temperature of the material. It might be inferred that at the zero of absolute temperature resistance would vanish altogether, and all pure metals become perfect conductors of electricity. This con-

clusion, however, has been rendered very doubtful by subsequent observations made at still lower temperatures, which appear to point to an ultimate finite resistance. Thus the temperature at which copper was assumed to have no resistance was *minus* 223 degrees, but that metal has been cooled to *minus* 253 degrees without getting rid of all resistance. The reduction in resistance of some of the metals at the boiling-point of hydrogen is very remarkable. Thus copper has only 1 per cent., gold and platinum 3 per cent., and silver 4 per cent. of the resistance they possessed at zero C., but iron still retains 12 per cent. of its initial resistance. In the case of alloys and impure metals, cold brings about a much smaller decrease in resistivity, and in the case of carbon and insulators like gutta-percha, glass, ebonite, &c., their resistivity steadily increases. The enormous increase in resistance of bismuth when transversely magnetised and cooled was also discovered in the course of these experiments. The study of dielectric constants at low temperatures has resulted in the discovery of some interesting facts. A fundamental deduction from Maxwell's theory is that the square of the refractive index of a body should be the same number as its dielectric constant. So far, however, from this being the case generally, the exceptions are far more numerous than the coincidences. It has been shown in the case of many substances, such as ice and glass, that an increase in the frequency of the alternating electromotive force results in a reduction of the dielectric constant to a value more consistent with Maxwell's law. By experiments upon many substances it is shown that even a moderate increase of frequency brings the large dielectric constant to values quite near to that required by Maxwell's law. It was thus shown that low temperature has the same effect as high frequency in annulling the abnormal dielectric values. The exact measurement of the dielectric constant of liquid oxygen as well as its magnetic permeability, combined with the optical determination of the refractive index, showed that liquid oxygen strictly obeys Maxwell's electro-optic law even at very low electric frequencies. In magnetic work the result of greatest value is the proof that magnetic susceptibility varies inversely as the absolute temperature. This shows that the magnetisation of paramagnetic bodies is an affair of orientation of molecules, and it suggests that at the absolute zero all the feebly paramagnetic bodies will be strongly magnetic. The diamagnetism of bismuth was found to be increased at low temperatures. The magnetic moment of a steel magnet is temporarily increased by cooling in liquid air, but the increase seems to have reached a limit, because on further cooling to the temperature of liquid hydrogen hardly any further change was observed. The study of the thermo-electric relations of the metals at low temperatures resulted in a great extension of the well-known Tait Thermo-Electric Diagram. Tait found that the thermo-electric power of the metals could be expressed by a linear function of the absolute temperature, but at the extreme range of temperature now under consideration this law was found not to hold generally; and further, it appeared that many abrupt

electric changes take place, which originate probably from specific molecular changes occurring in the metal. The thermo-electric neutral points of certain metals, such as lead and gold, which are located about or below the boiling-point of hydrogen, have been found to be a convenient means of defining specific temperatures in this exceptional part of the scale.

The effect of cold upon the life of living organisms is a matter of great intrinsic interest, as well as of wide theoretical importance. Experiment indicates that moderately high temperatures are much more fatal, at least to the lower forms of life, than are exceedingly low ones. Professor McKendrick froze for an hour at a temperature of 182° C. samples of meat, milk, &c., in sealed tubes; when these were opened after being kept at blood heat for a few days, their contents were found to be quite putrid. More recently some more elaborate tests were carried out at the Jenner Institute of Preventive Medicine on a series of typical bacteria. These were exposed to the temperature of liquid air for twenty hours, but their vitality was not affected, their functional activities remained unimpaired, and the cultures which they yielded were normal in every respect. The same result was obtained when liquid hydrogen was substituted for air. A similar persistence of life in seeds has been demonstrated even at the lowest temperatures; they were frozen for over a hundred hours in liquid air, at the instance of Messrs. Brown and Escombe, with no other result than to affect their protoplasm with a certain inertness, from which it recovered with warmth. Subsequently commercial samples of barley, pea, vegetable-marrow, and mustard seeds were literally steeped for six hours in liquid hydrogen at the Royal Institution, yet when they were sown by Sir W. T. Thiselton Dyer at Kew in the ordinary way, the proportion in which germination occurred was no less than in the other batches of the same seeds which had suffered no abnormal treatment. Bacteria are minute vegetable cells, the standard of measurement for which is the 'mikron.' Yet it has been found possible to completely triturate these microscopic cells, when the operation is carried out at the temperature of liquid air, the cells then being frozen into hard breakable masses. The typhoid organism has been treated in this way, and the cell plasma obtained for the purpose of studying its toxic and immunising properties. It would hardly have been anticipated that liquid air should find such immediate application in biological research. A research by Professor Macfadyen, just concluded, has shown that many varieties of micro-organisms can be exposed to the temperature of liquid air for a period of six months without any appreciable loss of vitality, although at such a temperature the ordinary chemical processes of the cell must cease. At such a temperature the cells cannot be said to be either alive or dead, in the ordinary acceptation of these words. It is a new and hitherto unobtainable condition of living matter—a third state. A final instance of the application of the above methods may be given. Certain species of bacteria during the course of their vital processes are capable of emitting

light. If, however, the cells be broken up at the temperature of liquid air, and the crushed contents brought to the ordinary temperature, the luminosity function is found to have disappeared. This points to the luminosity not being due to the action of a ferment—a 'Luciferase'—but as being essentially bound up with the vital processes of the cells, and dependent for its production on the intact organisation of the cell. These attempts to study by frigorific methods the physiology of the cell have already yielded valuable and encouraging results, and it is to be hoped that this line of investigation will continue to be vigorously prosecuted at the Jenner Institute.

And now, to conclude an address which must have sorely taxed your patience, I may remind you that I commenced by referring to the plaint of Elizabethan science, that cold was not a natural available product. In the course of a long struggle with nature, man, by the application of intelligent and steady industry, has acquired a control over this agency which enables him to produce it at will, and with almost any degree of intensity, short of a limit defined by the very nature of things. But the success in working what appears, at first sight, to be a quarry of research that would soon suffer exhaustion, has only brought him to the threshold of new labyrinths, the entanglements of which frustrate, with a seemingly invulnerable complexity, the hopes of further progress. In a legitimate sense all genuine scientific workers feel that they are 'the inheritors of unfulfilled renown.' The battlefields of science are the centres of a perpetual warfare, in which there is no hope of final victory, although partial conquest is ever triumphantly encouraging the continuance of the disciplined and strenuous attack on the seemingly impregnable fortress of Nature. To serve in the scientific army, to have shown some initiative, and to be rewarded by the consciousness that in the eyes of his comrades he bears the accredited accolade of successful endeavour, is enough to satisfy the legitimate ambition of every earnest student of Nature. The real warranty that the march of progress in the future will be as glorious as in the past lies in the perpetual reinforcement of the scientific ranks by recruits animated by such a spirit, and proud to obtain such a reward.

British Association for the Advancement of Science.

SOUTHPORT, 1903.

ADDRESS

BY

SIR NORMAN LOCKYER, K.C.B., LL.D., F.R.S.,
CORRESPONDANT DE L'INSTITUT DE FRANCE,
PRESIDENT.

The Influence of Brain-power on History.

My first duty to-night is a sad one. I have to refer to a great loss which this nation and this Association have sustained. By the death of the great Englishman and great statesman who has just passed away we members of the British Association are deprived of one of the most illustrious of our Past-Presidents. We have to mourn the loss of an enthusiastic student of science. We recognise that as Prime Minister he was mindful of the interests of science, and that to him we owe a more general recognition on the part of the State of the value to the nation of the work of scientific men. On all these grounds you will join in the expression of respectful sympathy with Lord Salisbury's family in their great personal loss which your Council has embodied this morning in a resolution of condolence.

Last year, when this friend of science ceased to be Prime Minister, he was succeeded by another statesman who also has given many proofs of his devotion to philosophical studies, and has shown in many utterances that he has a clear understanding of the real place of science in modern civilisation. We, then, have good grounds for hoping that the improvement in the position of science in this country which we owe to the one will also be the care of his successor, who has honoured the Association by accepting the unanimous nomination of your Council to be your President next year, an acceptance which adds a new lustre to this Chair.

On this we may congratulate ourselves all the more because I think, although it is not generally recognised, that the century into which we have now well entered may be more momentous than any which has preceded it, and that the present history of the world is being so largely moulded by the influence of brain-power, which in these modern days has to do with natural as well as human forces and laws, that statesmen and

politicians will have in the future to pay more regard to education and science as empire-builders and empire-guarders than they have paid in the past.

The nineteenth century will ever be known as the one in which the influences of science were first fully realised in civilised communities ; the scientific progress was so gigantic that it seems rash to predict that any of its successors can be more important in the life of any nation.

Disraeli, in 1873, referring to the progress up to that year, spoke as follows : ‘How much has happened in these fifty years—a period more remarkable than any, I will venture to say, in the annals of mankind. I am not thinking of the rise and fall of Empires, the change of dynasties, the establishment of Governments. I am thinking of those revolutions of science which have had much more effect than any political causes, which have changed the position and prospects of mankind more than all the conquests and all the codes and all the legislators that ever lived.’¹

The progress of science, indeed, brings in many considerations which are momentous in relation to the life of any limited community—any one nation. One of these considerations to which attention is now being greatly drawn is that a relative decline in national wealth derived from industries must follow a relative neglect of scientific education.

It was the late Prince Consort who first emphasised this when he came here fresh from the University of Bonn. Hence the ‘Prince Consort’s Committee,’ which led to the foundation of the College of Chemistry, and afterwards of the Science and Art Department. From that time to this the warnings of our men of science have become louder and more urgent in each succeeding year. But this is not all ; the commercial output of one country in one century as compared with another is not alone in question ; the acquirement of the scientific spirit and a knowledge and utilisation of the forces of Nature are very much further reaching in their effects on the progress and decline of nations than is generally imagined.

Britain in the middle of the last century was certainly the country which gained most by the advent of science, for she was then in full possession of those material gifts of Nature, coal and iron, the combined winning and utilisation of which, in the production of machinery and in other ways, soon made her the richest country in the world, the seat and throne of invention and manufacture, as Mr. Carnegie has called her. Being the great producers and exporters of all kinds of manufactured goods, we became eventually, with our iron ships, the great carriers, and hence the supremacy of our mercantile marine and our present command of the sea.

The most fundamental change wrought by the early applications of science was in relation to producing and carrying power. With the winning of mineral wealth and the production of machinery in other

¹ *Nature*, November 27, 1873, vol. ix. p. 71.

ADDRESS.

countries, and cheap and rapid transit between nations, our superiority as depending upon our first use of vast material resources was reduced. Science, which is above all things cosmopolitan—planetary, not national—internationalises such resources at once. In every market of the world

‘things of beauty, things of use,
Which one fair planet can produce,
Brought from under every star,’

were soon to be found.

Hence the first great effect of the general progress of science was relatively to diminish the initial supremacy of Britain due to the first use of *material* resources, which indeed was the real source of our national wealth and place among the nations.

The unfortunate thing was that, while the foundations of our superiority depending upon our *material resources* were being thus sapped by a cause *which was beyond our control*, our statesmen and our Universities were blind leaders of the blind, and our other asset, our mental resources, which was within our control, was culpably neglected.

So little did the bulk of our statesmen know of the part science was playing in the modern world and of the real basis of the nation's activities that they imagined political and fiscal problems to be the only matters of importance. Nor, indeed, are we very much better off to-day. In the important discussions recently raised by Mr. Chamberlain next to nothing has been said of the effect of the progress of science on prices. The whole course of the modern world is attributed to the presence or absence of taxes on certain commodities in certain countries. The fact that the great fall in the price of food-stuffs in England did not come till some thirty or forty years after the removal of the corn duty between 1847 and 1849 gives them no pause ; for them new inventions, railways, and steamships are negligible quantities ; the vast increase in the world's wealth, in Free Trade and Protected countries alike, comes merely, according to them, in response to some *political* shibboleth.

We now know, from what has occurred in other States, that if our Ministers had been more wise and our Universities more numerous and efficient our *mental resources* would have been developed by improvements in educational method, by the introduction of science into schools, and, more important than all the rest, by the teaching of science by experiment, observation, and research, and not from books. It is because this was not done that we have fallen behind other nations in properly applying science to industry, so that our applications of science to industry are relatively less important than they were. But this is by no means all ; we have lacked the strengthening of the national life produced by fostering the scientific spirit among all classes and along all lines of the nation's activity ; many of the responsible authorities know little and care less about science ; we have not learned that it is the duty of a State to organise its forces as carefully for peace as for war ; that Universities and

other teaching centres are as important as battleships or big battalions ; are, in fact, essential parts of a modern State's machinery, and, as such, to be equally aided and as efficiently organised to secure its future well-being.

Now the objects of the British Association as laid down by its founders seventy-two years ago are 'To give a stronger impulse and a more systematic direction to scientific inquiry—to promote the intercourse of those who cultivate science in different parts of the British Empire with one another and with foreign philosophers—to obtain a more general attention to the objects of science and a removal of any disadvantages of a public kind which impede its progress.'

In the main, my predecessors in this Chair, to which you have done me the honour to call me, have dealt, and with great benefit to science, with the objects first named.

But at a critical time like the present I find it imperative to depart from the course so generally followed by my predecessors and to deal with the last object named, for unless by some means or other we 'obtain a more general attention to the objects of science and a removal of any disadvantages of a public kind which impede its progress,' we shall suffer in competition with other communities in which science is more generally utilised for the purposes of the national life.

The Struggle for Existence in Modern Communities.

Some years ago, in discussing the relations of scientific instruction to our industries, Huxley pointed out that we were in presence of a new 'struggle for existence,' a struggle which, once commenced, must go on until only the fittest survives.

It is a struggle between organised species—nations—not between individuals or any class of individuals. It is, moreover, a struggle in which science and brains take the place of swords and sinews, on which depended the result of those conflicts which, up to the present, have determined the history and fate of nations. The school, the University, the laboratory, and the workshop are the battlefields of this new warfare.

But it is evident that if this, or anything like it, be true, our industries cannot be involved alone ; the scientific spirit, brain-power, must not be limited to the workshop, if other nations utilise it in all branches of their administration and executive.

It is a question of an important change of front. It is a question of finding a new basis of stability for the Empire in face of new conditions. I am certain that those familiar with the present state of things will acknowledge that the Prince of Wales's call, 'Wake up,' applies quite as much to the members of the Government as it does to the leaders of industry.

What is wanted is a complete organisation of the resources of the nation, so as to enable it best to face all the new problems which the

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progress of science, combined with the ebb and flow of population and other factors in international competition, are ever bringing before us. Every Minister, every public department, is involved; and this being so, it is the duty of the whole nation—King, Lords, and Commons—to do what is necessary to place our scientific institutions on a proper footing in order to enable us to ‘face the music,’ whatever the future may bring. The idea that science is useful only to our industries comes from want of thought. If anyone is under the impression that Britain is only suffering at present from the want of the scientific spirit among our industrial classes, and that those employed in the State service possess adequate brain-power and grip of the conditions of the modern world into which science so largely enters, let him read the Report of the Royal Commission on the War in South Africa. There he will see how the whole ‘system’ employed was, in Sir Henry Brackenbury’s words applied to a part of it, ‘*unsuited to the requirements of an army which is maintained to enable us to make war.*’ Let him read also in the Address of the President of the Society of Chemical Industry what drastic steps had to be taken by Chambers of Commerce and ‘a quarter of a million of working-men’ to get the Patent Law Amendment Act into proper shape in spite of all the advisers and officials of the Board of Trade. Very few people realise the immense number of scientific problems the solution of which is required for the State service. The nation itself is a gigantic workshop; and the more our rulers and legislators, administrators and executive officers possess the scientific spirit, the more the rule of thumb is replaced in the State service by scientific methods, the more able shall we be, thus armed at all points, to compete successfully with other countries along all lines of national as well as of commercial activity.

It is obvious that the power of a nation for war, in men and arms and ships, is one thing; its power in the peace struggles to which I have referred is another. In the latter the source and standard of national efficiency are entirely changed. To meet war conditions, there must be equality or superiority in battleships and army corps. To meet the new peace conditions, there must be equality or superiority in Universities, scientific organisation, and everything which conduces to greater brain-power.

Our Industries are suffering in the present International Competition.

The present condition of the nation, so far as its industries are concerned, is as well known, not only to the Prime Minister, but to other political leaders in and out of the Cabinet, as it is to you and to me. Let me refer to two speeches delivered by Lord Rosebery and Mr. Chamberlain on two successive days in January 1901.

Lord Rosebery spoke as follows:—

‘. . . The war I regard with apprehension is the war of trade which is unmistakably upon us. . . . When I look round me I cannot blind my

eyes to the fact that, so far as we can predict anything of the twentieth century on which we have now entered, it is that it will be one of acutest international conflict in point of trade. We were the first nation of the modern world to discover that trade was an absolute necessity. For that we were nicknamed a nation of shopkeepers ; but now every nation wishes to be a nation of shopkeepers too, and I am bound to say that when we look at the character of some of these nations, and when we look at the intelligence of their preparations, we may well feel that it behoves us not to fear, but to gird up our loins in preparation for what is before us.'

Mr. Chamberlain's views were stated in the following words :—

'I do not think it is necessary for me to say anything as to the urgency and necessity of scientific training. . . . It is not too much to say that the existence of this country, as the great commercial nation, depends upon it. . . . It depends very much upon what we are doing now, at the beginning of the twentieth century, whether at its end we shall continue to maintain our supremacy or even equality with our great commercial and manufacturing rivals.'

All this refers to our industries. We are suffering because trade no longer follows the flag as in the old days, but because trade follows the brains, and our manufacturers are too apt to be careless in securing them. In one chemical establishment in Germany 400 doctors of science, the best the Universities there can turn out, have been employed at different times in late years. In the United States the most successful students in the higher teaching centres are snapped up the moment they have finished their course of training, and put into charge of large concerns, so that the idea has got abroad that youth is the password of success in American industry. It has been forgotten that the latest product of the highest scientific education must necessarily be young, and that it is the training and not the age which determines his employment. In Britain, on the other hand, apprentices who can pay high premiums are too often preferred to those who are well educated, and the old rule-of-thumb processes are preferred to new developments—a conservatism too often depending upon the master's own want of knowledge.

I should not be doing my duty if I did not point out that the defeat of our industries one after another, concerning which both Lord Rosebery and Mr. Chamberlain express their anxiety, is by no means the only thing we have to consider. The matter is not one which concerns our industrial classes only, for knowledge must be pursued for its own sake ; and since the full life of a nation with a constantly increasing complexity, not only of industrial, but of high national aims, depends upon the universal presence of the scientific spirit—in other words, brain-power—our whole national life is involved.

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The Necessity for a Body dealing with the Organisation of Science.

The present awakening in relation to the nation's real needs is largely due to the warnings of men of science. But Mr. Balfour's terrible Manchester picture of our present educational condition¹ shows that the warning, which has been going on now for more than fifty years, has not been forcible enough; but if my contention that other reorganisations besides that of our education are needed is well founded, and if men of science are to act the part of good citizens in taking their share in endeavouring to bring about a better state of things, the question arises, Has the neglect of their warnings so far been due to the way in which these have been given?

Lord Rosebery, in the address to a Chamber of Commerce from which I have already quoted, expressed his opinion that such bodies do not exercise so much influence as might be expected of them. But if commercial men do not use all the power their organisation provides, do they not by having built up such an organisation put us students of science to shame, who are still the most disorganised members of the community?

Here, in my opinion, we have the real reason why the scientific needs of the nation fail to command the attention either of the public or of successive Governments. At present, appeals on this or on that behalf are the appeals of individuals; science has no collective voice on the larger national questions; there is no organised body which formulates her demands.

During many years it has been part of my duty to consider such matters, and I have been driven to the conclusion that our great crying need is to bring about an organisation of men of science and all interested in science similar to those which prove so effective in other branches of human activity. For the last few years I have dreamt of a Chamber, Guild, League, call it what you will, with a wide and large membership, which should give us what, in my opinion, is so urgently needed. Quite recently I sketched out such an organisation, but what was my astonishment to find that I had been forestalled, and by the founders of the British Association!

The British Association such a Body.

At the commencement of this Address I pointed out that one of the objects of the Association, as stated by its founders, was 'to obtain a more general attention to the objects of science and a removal of any disadvantages of a public kind which impede its progress.'

Everyone connected with the British Association from its beginning

¹ 'The existing educational system of this country is chaotic, is ineffectual, is utterly behind the age, makes us the laughing-stock of every advanced nation in Europe and America, puts us behind, not only our American cousins, but the German and the Frenchman and the Italian.'—*Times*, October 15, 1902.

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may be congratulated upon the magnificent way in which the other objects of the Association have been carried out ; but as one familiar with the Association for the last forty years I cannot but think that the object to which I have specially referred has been too much overshadowed by the work done in connection with the others.

A careful study of the early history of the Association leads me to the belief that the function I am now dwelling on was strongly in the minds of the founders ; but be this as it may, let me point out how admirably the organisation is framed to enable men of science to influence public opinion and so to bring pressure to bear upon Governments which follow public opinion. (1) Unlike all the other chief metropolitan societies, its outlook is not limited to any branch or branches of science. (2) We have a wide and numerous fellowship, including both the leaders and the lovers of science, in which all branches of science are and always have been included with the utmost catholicity—a condition which renders strong committees possible on any subject. (3) An annual meeting at a time when people can pay attention to the deliberations, and when the newspapers can print reports. (4) The possibility of beating up recruits and establishing local committees in different localities, even in the King's dominions beyond the seas, since the place of meeting changes from year to year, and is not limited to these islands.

We not only, then, have a scientific Parliament competent to deal with all matters, including those of national importance, relating to science, but machinery for influencing all new councils and committees dealing with local matters, the functions of which are daily becoming more important.

The machinery might consist of our corresponding societies. We already have affiliated to us seventy societies with a membership of 25,000. Were this number increased so as to include every scientific society in the Empire, metropolitan and provincial, we might eventually hope for a membership of half a million.

I am glad to know that the Council is fully alive to the importance of giving a greater impetus to the work of the corresponding societies. During this year a committee was appointed to deal with the question ; and later still, after this committee had reported, a conference was held between this committee and the corresponding societies committee to consider the suggestions made, some of which will be gathered from the following extract :—

‘In view of the increasing importance of science to the nation at large, your committee desire to call the attention of the Council to the fact that in the corresponding societies the British Association has gathered in the various centres represented by these societies practically all the scientific activity of the provinces. The number of members and associates at present on the list of the corresponding societies approaches 25,000, and no organisation is in existence anywhere in the country better adapted

than the British Association for stimulating, encouraging, and co-ordinating all the work being carried on by the seventy societies at present enrolled. Your committee are of opinion that further encouragement should be given to these societies and their individual working members by every means within the power of the Association ; and with the object of keeping the corresponding societies in more permanent touch with the Association they suggest that an official invitation on behalf of the Council be addressed to the societies, through the corresponding societies committee, asking them to appoint standing British Association sub-committees, to be elected by themselves, with the object of dealing with all those subjects of investigation common to their societies and to the British Association committees, and to look after the general interests of science and scientific education throughout the provinces and provincial centres. . . .

‘ Your committee desire to lay special emphasis on the necessity for the extension of the scientific activity of the corresponding societies and the expert knowledge of many of their members in the direction of scientific education. They are of opinion that immense benefit would accrue to the country if the corresponding societies would keep this requirement especially in view with the object of securing adequate representation for scientific education on the Education Committees now being appointed under the new Act. The educational section of the Association having been but recently added, the corresponding societies have as yet not had much opportunity for taking part in this branch of the Association’s work ; and in view of the reorganisation in education now going on all over the country your committee are of opinion that no more opportune time is likely to occur for the influence of scientific organisations to make itself felt as a real factor in national education. . . . ’

I believe that if these suggestions or anything like them—for some better way may be found on inquiry—are accepted, great good to science throughout the Empire will come. Rest assured that sooner or later such a Guild will be formed because it is needed. It is for you to say whether it shall be, or form part of, the British Association. We in this Empire certainly need to organise science as much as in Germany they find the need to organise a navy. The German Navy League, which has branches even in our Colonies, already has a membership of 630,000, and its income is nearly 20,000*l.* a year. A British Science League of 500,000 with a sixpenny subscription would give us 12,000*l.* a year, quite enough to begin with.

I for one believe that the British Association would be a vast gainer by such an expansion of one of its existing functions. Increased authority and prestige would follow its increased utility. The meetings would possess a new interest ; there would be new subjects for reports ; missionary work less needed than formerly would be replaced by efforts much more suited to the real wants of the time. This magnificent, strong, and complicated organisation would become a living force, working throughout the

year instead of practically lying idle, useless, and rusting for fifty-one weeks out of the fifty-two so far as its close association with its members is concerned.

If this suggestion in any way commends itself to you, then when you begin your work in your sections or General Committee see to it that a body is appointed to inquire how the thing can be done. Remember that the British Association will be as much weakened by the creation of a new body to do the work I have shown to have been in the minds of its founders as I believe it will be strengthened by becoming completely effective in every one of the directions they indicated, and for which effectiveness we, their successors, are indeed responsible. The time is appropriate for such a reinforcement of one of the wings of our organisation, for we have recently included Education among our sections.

There is another matter I should like to see referred to the committee I have spoken of, if it please you to appoint it. The British Association—which, as I have already pointed out, is now the chief body in the Empire which deals with the totality of science—is, I believe, the only organisation of any consequence which is without a charter, and which has not his Majesty the King as patron.

The First Work of such an Organisation.

I suppose it is my duty, after I have suggested the need of an organisation, to tell you my personal opinion as to the matters where we suffer most in consequence of our lack of organisation at the present time.

Our position as a nation, our success as merchants, are in peril chiefly—dealing with preventable causes—because of our lack of completely efficient Universities and our neglect of research. This research has a double end. A professor who is not learning cannot teach properly or arouse enthusiasm in his students; while a student of anything who is unfamiliar with research methods, and without that training which research brings, will not be in the best position to apply his knowledge in after-life. From neglect of research comes imperfect education and a small output of new applications and new knowledge to reinvigorate our industries. From imperfect education comes the unconcern touching scientific matters and the too frequent absence of the scientific spirit in the nation generally, from the Court to the Parish Council.

I propose to deal as briefly as I can with each of these points.

Universities.

I have shown that, so far as our industries are concerned, the cause of our failure has been run to earth; it is fully recognised that it arises from the insufficiency of our Universities both in numbers and efficiency, so that not only our captains of industry, but those employed in the nation's work generally, do not secure a training similar to that afforded by other nations. No additional endowment of primary, secondary, or

technical instruction will mend matters. This is not merely the opinion of men of science ; our great towns know it, our Ministers know it.

It is sufficient for me to quote Mr. Chamberlain :—

‘It is not everyone who can, by any possibility, go forward into the higher spheres of education ; but it is from those who do that we have to look for the men who in the future will carry high the flag of this country in commercial, scientific, and economic competition with other nations. At the present moment I believe there is nothing more important than to supply the deficiencies which separate us from those with whom we are in the closest competition. In Germany, in America, in our own colony of Canada, and in Australia, the higher education of the people has more support from the Government, is carried further, than it is here in the Old Country ; and the result is that in every profession, in every industry, you find the places taken by men and by women who have had a University education. And I would like to see the time in this country when no man should have a chance for any occupation of the better kind, either in our factories, our workshops, or our counting-houses, who could not show proof that in the course of his University career he had deserved the position that was offered to him. What is it that makes a country ? Of course you may say, and you would be quite right, “The general qualities of the people, their resolution, their intelligence, their pertinacity, and many other good qualities.” Yes ; but that is not all, and it is not the main creative feature of a great nation. The greatness of a nation is made by its greatest men. It is those we want to educate. It is to those who are able to go, it may be, from the very lowest steps in the ladder, to men who are able to devote their time to higher education, that we have to look to continue the position which we now occupy as at all events one of the greatest nations on the face of the earth. And, feeling as I do on these subjects, you will not be surprised if I say that I think the time is coming when Governments will give more attention to this matter, and perhaps find a little more money to forward its interests.’¹

Our conception of a University has changed. University education is no longer regarded as the luxury of the rich, which concerns only those who can afford to pay heavily for it. The Prime Minister in a recent speech, while properly pointing out that the collective effect of our public and secondary schools upon British character cannot be overrated, frankly admitted that the boys of seventeen or eighteen who have to be educated in them ‘do not care a farthing about the world they live in except in so far as it concerns the cricket-field or the football-field or the river.’ On this ground they are not to be taught science ; and hence, when they proceed to the University, their curriculum is limited to subjects which were better taught before the modern world existed, or even Galileo

¹ *Times*, November 6, 1902.

was born. But the science which these young gentlemen neglect, with the full approval of their teachers, on their way through the school and the University to politics, the Civil Service, or the management of commercial concerns, is now one of the great necessities of a nation ; and our Universities must become as much the insurers of the future progress as battleships are the insurers of the present power of States. In other words, University competition between States is now as potent as competition in building battleships ; and it is on this ground that our University conditions become of the highest national concern, and therefore have to be referred to here, and all the more because our industries are not alone in question.

Why we have not more Universities.

Chief among the causes which have brought us to the terrible condition of inferiority as compared with other nations in which we find ourselves are our carelessness in the matter of education and our false notions of the limitations of State functions in relation to the conditions of modern civilisation.

Time was when the Navy was largely a matter of private and local effort. William the Conqueror gave privileges to the Cinque Ports on the condition that they furnished fifty-two ships when wanted. In the time of Edward III., of 730 sail engaged in the siege of Calais 705 were 'people's ships.' All this has passed away ; for our first line of defence we no longer depend on private and local effort.

Time was when not a penny was spent by the State on elementary education. Again, we no longer depend upon private and local effort. The Navy and primary education are now recognised as properly calling upon the public for the necessary financial support. But when we pass from primary to University education, instead of State endowment we find State neglect ; we are in a region where it is nobody's business to see that anything is done.

We in Great Britain have thirteen Universities competing with 134 State and privately endowed in the United States and twenty-two State-endowed in Germany. I leave other countries out of consideration for lack of time, and I omit all reference to higher institutions for technical training, of which Germany alone possesses nine of University rank, because they are less important ; they instruct rather than educate, and our want is education. The German State gives to one University more than the British Government allows to all the Universities and University Colleges in England, Ireland, Scotland, and Wales put together. These are the conditions which regulate the production of brain-power in the United States, Germany, and Britain respectively, and the excuse of the Government is that this is a matter for private effort. Do not our Ministers of State know that other civilised countries grant efficient State aid, and, further, that private effort has provided in Great Britain less than 10 per cent. of the sum thus furnished in the United States in

addition to State aid? Are they content that we should go under in the great struggle of the modern world because the Ministries of other States are wiser, and because the individual citizens of another country are more generous, than our own?

If we grant that there was some excuse for the State's neglect so long as the higher teaching dealt only with words, and books alone had to be provided (for the streets of London and Paris have been used as class-rooms at a pinch), it must not be forgotten that during the last hundred years not only has knowledge been enormously increased, but things have replaced words, and fully equipped laboratories must take the place of books and class-rooms if University training worthy of the name is to be provided. There is much more difference in size and kind between an old and a new University than there is between the old caravel and a modern battleship, and the endowments must follow suit.

What are the facts relating to private endowment in this country? In spite of the munificence displayed by a small number of individuals in some localities, the truth must be spoken. In depending in our country upon this form of endowment we are trusting to a broken reed. If we take the twelve English University Colleges, the forerunners of Universities unless we are to perish from lack of knowledge, we find that private effort during sixty years has found less than 4,000,000% ; that is, 2,000,000% for buildings, and 40,000% a year income. This gives us an average of 166,000% for buildings, and 3,300% for yearly income.

What is the scale of private effort we have to compete with in regard to the American Universities?

In the United States, during the last few years, Universities and colleges have received more than 40,000,000% from this source alone ; private effort supplied nearly 7,000,000% in the years 1898-1900.

Next consider the amount of State aid to Universities afforded in Germany. The buildings of the new University of Strassburg have already cost nearly a million ; that is, about as much as has yet been found by private effort for buildings in Manchester, Liverpool, Birmingham, Bristol, Newcastle, and Sheffield. The Government annual endowment of the same German University is more than 49,000%.

This is what private endowment does for us in England, against State endowment in Germany.

But the State does really concede the principle ; its present contribution to our Universities and colleges amounts to 155,600% a year. No capital sum, however, is taken for buildings. The State endowment of the University of Berlin in 1891-2 amounted to 168,777%.

When, then, we consider the large endowments of University education both in the United States and Germany, it is obvious that State aid only can make any valid competition possible with either. The more we study the facts, the more statistics are gone into, the more do we find that we, to a large extent, lack both of the sources of endowment upon one or other, or both, of which other nations depend. We are between

two stools, and the prospect is hopeless without some drastic changes. And first among these, if we intend to get out of the present Slough of Despond, must be the giving up of the idea of relying upon private effort.

That we lose most where the State does least is known to Mr. Chamberlain, for in his speech, to which I have referred, on the University of Birmingham, he said : ‘ As the importance of the aim we are pursuing becomes more and more impressed upon the minds of the people, we may find that we shall be more generously treated by the State.’

Later still, on the occasion of a visit to University College School, Mr. Chamberlain spoke as follows :—

‘ When we are spending, as we are, many millions—I think it is 13,000,000*l.*—a year on primary education, it certainly seems as if we might add a little more, even a few tens of thousands, to what we give to University and secondary education.’¹

To compete on equal grounds with other nations we must have more Universities. But this is not all—we want a far better endowment of all the existing ones, not forgetting better opportunities for research on the part of both professors and students. Another crying need is that of more professors and better pay. Another is the reduction of fees ; they should be reduced to the level existing in those countries which are competing with us—to, say, one-fifth of their present rates, so as to enable more students in the secondary and technical schools to complete their education.

In all these ways facilities would be afforded for providing the highest instruction to a much greater number of students. At present there are almost as many *professors and instructors* in the Universities and colleges of the United States as there are *day students* in the Universities and colleges of the United Kingdom.

Men of science, our leaders of industry, and the chiefs of our political parties all agree that our present want of higher education—in other words, properly equipped Universities—is heavily handicapping us in the present race for commercial supremacy, because it provides a relatively inferior brain-power, which is leading to a relatively reduced national income.

The facts show that in this country we cannot depend upon private effort to put matters right. How about local effort ?

Anyone who studies the statistics of modern municipalities will see that it is impossible for them to raise rates for the building and upkeep of Universities.

The buildings of the most modern University in Germany have cost a million. For upkeep the yearly sums found, chiefly by the State, for

¹ *Times*, November 6, 1902.

German Universities of different grades, taking the incomes of seven out of the twenty-two Universities as examples, are :—

First Class .	Berlin	£ 130,000
Second Class.	{ Bonn	56,000
	{ Göttingen	
Third Class .	{ Königsberg	48,000
	{ Strassburg	
Fourth Class.	{ Heidelberg	37,000
	{ Marburg	

Thus, if Leeds, which is to have a University, is content with the fourth class German standard, a rate must be levied of 7*d.* in the pound for yearly expenses, independent of all buildings. But the facts are that our towns are already at the breaking strain. During the last fifty years, in spite of enormous increases in rateable values, the rates have gone up from about 2*s.* to about 7*s.* in the pound for real *local* purposes. But no University can be a merely local institution.

How to get more Universities.

What, then, is to be done? Fortunately, we have a precedent admirably in point, the consideration of which may help us to answer this question.

I have pointed out that in old days our Navy was chiefly provided by local and private effort. Fortunately for us those days have passed away; but some twenty years ago, in spite of a large expenditure, it began to be felt by those who knew, that in consequence of the increase of foreign navies our sea-power was threatened, as now, in consequence of the increase of foreign Universities, our brain-power is threatened.

The nation slowly woke up to find that its enormous commerce was no longer insured at sea, that in relation to foreign navies our own had been suffered to dwindle to such an extent that it was no longer capable of doing the duty which the nation expected of it even in times of peace. At first this revelation was received with a shrug of incredulity, and the peace-at-any-price party denied that anything was needed; but a great teacher arose;¹ as the facts were inquired into, the suspicion changed into an alarm; men of all parties saw that something must be done. Later the nation was thoroughly aroused, and with an universal agreement the principle was laid down that, cost what it might to enforce our sea-power, our Navy must be made and maintained of a strength greater than those of any two possibly contending Powers. After establishing this principle, the next thing to do was to give effect to it. What did the nation do after full discussion and inquiry? A Bill was brought in in 1888, and a sum of 21,500,000*l.* was voted in order, during the next five years, to inaugurate a large ship-building programme,

¹ Captain Mahan, of the U.S. Navy, whose book, 'On the Influence of Sea-power on History,' has suggested the title of my address.

so that Britain and Britain's commerce might be guarded on the high seas in any event.

Since then we have spent 120,000,000*l.* on new ships, and this year we spend still more millions on still more new ships. If these prove insufficient to safeguard our sea-power, there is no doubt that the nation will increase them, and I have not heard that anybody has suggested an appeal to private effort.

How, then, do we stand with regard to Universities, recognising them as the chief producers of brain-power and therefore the equivalents of battleships in relation to sea-power? Do their numbers come up to the standard established by the Admiralty principle to which I have referred? Let us attempt to get a rough-and-ready estimate of our educational position by counting Universities as the Admiralty counts battleships. I say rough-and-ready, because we have other helps to greater brain-power to consider besides Universities, as the Admiralty has other ships to consider besides ironclads.

In the first place, let us inquire if they are equal in number to those of any two nations commercially competing with us.

In the United Kingdom we had until quite recently thirteen.¹ Of these, one is only three years old as a teaching University, and another is still merely an examining board.

In Germany there are twenty-two Universities; in France, under recent legislation, fifteen; in Italy, twenty-one. It is difficult to give the number in the United States, because it is clear, from the tables given in the Report of the Commissioner of Education, that some colleges are more important than some Universities, and both give the degree of Ph.D. But of Universities in title we have 134. Among these, there are forty-six with more than fifty professors and instructors, and thirteen with more than 150. I will take that figure.

Suppose we consider the United States and Germany, our chief commercial competitors, and apply the Admiralty principle. We should require, allowing for population, eight additional Universities at the very lowest estimate.

We see, then, that instead of having Universities equalling in number those of two of our chief competitors together, they are by no means equal to those of either of them singly.

After this statement of the facts, anyone who has belief in the importance of higher education will have no difficulty in understanding the origin of the present condition of British industry and its constant decline, first in one direction and then in another, since the tremendous efforts made in the United States and Germany began to take effect.

If, indeed, there be anything wrong about the comparison, the error can only arise from one of two sources—either the Admiralty is thought-

¹ These are Oxford, Cambridge, Durham, Victoria, Wales, Birmingham, London, St. Andrews, Glasgow, Aberdeen, Edinburgh, Dublin, and Royal University.

lessly and wastefully spending money, or there is no connection whatever between the higher intelligence and the prosperity of a nation. I have already referred to the views of Mr. Chamberlain and Lord Rosebery on this point; we know what Mr. Chamberlain has done at Birmingham; we know the strenuous efforts made by the commercial leaders of Manchester and Liverpool; we know, also, the opinion of men of science.

If while we spend so freely to maintain our sea-power our export of manufactured articles is relatively reduced because our competitors beat us in the markets of the world, what is the end of the vista thus opened up to us? A Navy growing stronger every year and requiring larger votes to guard our commerce and communications, and a vanishing quantity of commerce to guard—a reduced national income to meet an increasing taxation!

The pity is that our Government has considered sea-power alone; that while so completely guarding our commerce it has given no thought to one of the main conditions on which its production and increase depend. A glance could have shown that other countries were building Universities even faster than they were building battleships; were, in fact, considering brain-power first and sea-power afterwards.

Surely it is my duty as your President to point out the danger ahead, if such ignoring of the true situation should be allowed to continue. May I express a hope that at last, in Mr. Chamberlain's words, 'The time is coming when Governments will give more attention to this matter'?

What will they cost?

The comparison shows that we want eight new Universities, some of which, of course, will be colleges promoted to University rank and fitted to carry on University work. Three of them are already named: Manchester, Liverpool, Leeds.

Let us take this number and deal with it on the battleship condition, although a modern University on American or German models will cost more to build than a battleship.

If our present University shortage be dealt with on battleship conditions, to correct it we should expend *at least* 8,000,000*l.* for new construction, and for the pay-sheet we should have to provide ($8 \times 50,000$ *l.*) 400,000 *l.* yearly for *personnel* and up-keep; for it is of no use to build either ships or Universities without manning them. Let us say, roughly, capitalising the yearly payment at $2\frac{1}{2}$ per cent., 24,000,000 *l.*

At this stage it is important to inquire whether this sum, arrived at by analogy merely, has any relation to our real University needs.

I have spent a year in making inquiries, as full as I could make them, of friends conversant with the real present needs of each of the Universities, old and new. I have obtained statistics which would fill a volume, and personally I believe that this sum at least is required to bring our

University system up to anything like the level which is insisted upon both in the United States and in Germany. Even Oxford, our oldest University, will still continue to be a mere bundle of colleges unless three millions are provided to enable the University, properly so called, to take her place among her sisters of the modern world; and Sir Oliver Lodge, the Principal of our very youngest University, Birmingham, has shown in detail how five millions can be usefully and properly applied in that one locality to utilise for the good of the nation the enthusiasm and scientific capacity which are only waiting for adequate opportunity of development.

How is this money to be raised? I reply, without hesitation, *Duplicate the Navy Bill of 1888-9*; do at once for brain-power what we so successfully did then for sea-power.

Let 24,000,000*l.* be set apart from one asset, our national wealth, to increase the other, brain-power. Let it be assigned and borrowed as it is wanted; there will be a capital sum for new buildings to be erected in the next five or ten years, the interest of the remainder to go towards increased annual endowments.

There need be no difficulty about allocating money to the various institutions. Let each University make up its mind as to which rank of the German Universities it wishes to emulate. When this claim has been agreed to, the sums necessary to provide the buildings and teaching staff of that class of University should be granted without demur.

It is the case of battleships over again, and money need not be spent more freely in one case than in the other.

Let me at once say that this sum is not to be regarded as practically gone when spent, as in the case of a short-lived ironclad. *It is a loan* which will bear a high rate of interest. This is not my opinion merely; it is the opinion of those concerned in great industrial enterprises and fully alive to the origin and effects of the present condition of things.

I have been careful to point out that the statement that our industries are suffering from our relative neglect of science does not rest on my authority. But if this be true, then if our annual production is less by only two millions than it might have been, having two millions less to divide would be equivalent to our having forty or fifty millions less capital than we should have had if we had been more scientific.

Sir John Brunner, in a speech connected with the Liverpool School of Tropical Medicine, stated recently that if we as a nation were now to borrow ten millions of money in order to help science by putting up buildings and endowing professors, we should get the money back in the course of a generation a hundredfold. He added that there was no better investment for a business man than the encouragement of science, and that every penny he possessed had come from the application of science to commerce.

According to Sir Robert Giffen, the United Kingdom as a going concern was in 1901 worth 16,000,000,000*l.*

Were we to put aside 24,000,000*l.* for gradually organising, building, and endowing new Universities, and making the existing ones more efficient, we should still be worth 15,976,000,000*l.*—a property well worth defending by all the means, and chief among these brain-power, we can command.

If it be held that this, or anything like it, is too great a price to pay for correcting past carelessness or stupidity, the reply is that the 120,000,000*l.* recently spent on the Navy, a sum five times greater, has been spent to correct a sleepy blunder, not one whit more inimical to the future welfare of our country than that which has brought about our present educational position. We had not sufficiently recognised what other nations had done in the way of ship-building, just as until now we have not recognised what they have been doing in University building.

Further, I am told that the sum of 24,000,000*l.* is less than half the amount by which Germany is yearly enriched by having improved upon our chemical industries, owing to our lack of scientific training. Many other industries have been attacked in the same way since ; but taking this one instance alone, if we had spent this money fifty years ago, when the Prince Consort first called attention to our backwardness, the nation would now be much richer than it is, and would have much less to fear from competition.

Suppose we were to set about putting our educational house in order, so as to secure a higher quality and greater quantity of brain-power, it would not be the first time in history that this has been done. Both Prussia after Jena and France after Sedan acted on the view :—

‘ When land is gone and money spent,
Then learning is most excellent.’

After Jena, which left Prussia a ‘ bleeding and lacerated mass,’ the King and his wise counsellors, among them men who had gained knowledge from Kant, determined, as they put it, ‘ to supply the loss of territory by intellectual effort.’

What did they do ? In spite of universal poverty, three Universities, to say nothing of observatories and other institutions, were at once founded, secondary education was developed, and in a few years the mental resources were so well looked after that Lord Palmerston defined the kingdom in question as ‘ a country of damned professors.’

After Sedan—a battle, as Moltke told us, ‘ won by the schoolmaster ’—France made even more strenuous efforts. The old University of France, with its ‘ academies ’ in various places, was replaced by fifteen independent Universities, in all of which are faculties of letters, sciences, law and medicine.

The development of the University of Paris has been truly marvellous. In 1897–8 there were 12,000 students, and the cost was 200,000*l.* a year.

But even more wonderful than these examples is the ‘ intellectual effort ’ made by Japan, not after a war, but to prepare for one.

The question is, Shall we wait for a disaster and then imitate Prussia and France ; or shall we follow Japan and thoroughly prepare by 'intellectual effort' for the industrial struggle which lies before us ?

Such an effort seems to me to be the first thing any national or imperial scientific organisation should endeavour to bring about.

Research.

When dealing with our Universities I referred to the importance of research, as it is now generally acknowledged to be the most powerful engine of education that we possess. But education, after all, is but a means to the end, which, from the national point of view, is the application of old and the production of new knowledge.

Its national importance apart from education is now so generally recognised that in all civilised nations except our own means of research are being daily more amply provided for all students after they have passed through their University career ; and, more than this, for all who can increase the country's renown or prosperity by the making of new knowledge, upon which not only commercial progress, but all intellectual advance must depend.

I am so anxious that my statement of our pressing, and indeed imperative, needs in this direction should not be considered as resting upon the possibly interested opinion of a student of science merely that I must trouble you with still more quotations.

Listen to Mr. Balfour :—

'I do not believe that any man who looks round the equipment of our Universities or medical schools or other places of education can honestly say in his heart that we have done enough to equip research with all the costly armoury which research must have in these modern days. We, the richest country in the world, lag behind Germany, France, Switzerland, and Italy. Is it not disgraceful ? Are we too poor or are we too stupid ?'¹

It is imagined by many who have given no thought to the matter that this research should be closely allied with some application of science being utilised at the time. Nothing could be further from the truth ; nothing could be more unwise than such a limitation.

Surely all the laws of Nature will be ultimately of service, and therefore there is much more future help to be got from a study of the unknown and the unused than we can hope to obtain by continuing the study of that which is pretty well known and utilised already. It was a King of France, Louis XIV., who first commended the study of the *même inutile*. The history of modern science shows us more and more as the years roll on the necessity and advantage of such studies, and therefore the importance of properly endowing them ; for the production of new knowledge is a costly and unremunerative pursuit.

¹ *Nature*, May 30, 1901.

Years ago we had Faraday apparently wasting his energies and time in playing with needles ; electricity now fills the world. To-day men of science in all lands are studying the emanations of radium ; no research could be more abstract ; but who knows what advance in human thought may follow or what gigantic world-transforming superstructure may eventually be raised on the minute foundation they are laying ?

If we so organise our teaching forces that we can use them at all stages, from the gutter to the University, to sift out for us potential Faradays—to utilise the mental products which otherwise would be wasted—it is only by enabling such men to continue their learning after their teaching is over that we shall be able to secure the greatest advantage which any educational system can afford.

It is now more than thirty years ago that my attention was specially drawn to this question of the endowment of research—first, by conversations with M. Dumas, the permanent secretary of the Academy of Sciences, who honoured me by his friendship ; and, secondly, by my association with Sir Benjamin Brodie and Dr. Appleton in their endeavours to call attention to the matter in this country. At that time a general scheme of endowment suggested by Dumas was being carried out by Duruy. This took the form of the ‘*École spéciale des Hautes Études*’ ; it was what our fellowship system was meant to be—an endowment of the research of post-graduate students in each seat of learning. The French effort did not begin then.

I may here tell, as it was told me by Dumas, the story of Léon Foucault, whose many discoveries shed a glory on France and revived French industry in many directions.¹ In 1851, when Prince Napoleon was President of the Republic, he sent for Dumas and some of his colleagues, and told them that during his stay in England, and afterwards in his study of the Great Exhibition of that year, he had found there a greater industrial development than in France, and more applications of science, adding that he wished to know how such a state of things could be at once remedied. The answer was that new applications depended upon new knowledge, and that therefore the most direct and immediate way was to find and encourage men who were likely by research in pure science to produce this new knowledge. The Prince-President at once asked for names ; that of Léon Foucault was the only one mentioned during the first interview.

Some time afterwards—to be exact, at about eleven in the morning of December 2—Dumas’s servant informed him that there was a gentleman in the hall named Foucault, who wished to see him, and he added that he appeared to be very ill. When shown into the study, Foucault was too agitated to speak, and was blind with tears. His reply to Dumas’s soothing questions was to take from his pockets two rolls of

¹ See *Proc. R. S.* vol. xvii. p. lxxxiii.

banknotes, amounting to 200,000 francs, and place them on the table. Finally, he was able to say that he had been with the Prince-President since eight o'clock that morning, discussing the possible improvement of French science and industry; and that Napoleon had finally given him the money, requesting him to do all in his power to aid the State. Foucault ended by saying that, on realising the greatness of the task thus imposed upon him, his fears and feelings had got the better of him, for the responsibility seemed more than he could bear.¹

The movement in England to which I have referred began in 1872, when a society for the organisation of academical study was formed in connection with the inquiry into the revenues of Oxford and Cambridge, and there was a famous meeting at the Freemasons' Tavern, Mark Pattison being in the chair. Brodie, Rolleston, Carpenter, Burdon-Sanderson, were among the speakers, and the first resolution carried was, 'That to have a class of men whose lives are devoted to research is a national object.' The movement died in consequence of the want of sympathy of the University authorities.²

In the year 1874 the subject was inquired into by the late Duke of Devonshire's Commission; and after taking much remarkable evidence, including that of Lord Salisbury, the Commission recommended to the Government that the then grant of 1,000*l.*, which was expended, by a committee appointed by the Royal Society, on instruments needed in researches carried on by private individuals, should be increased, so that personal grants should be made. This recommendation was accepted and acted on; the grant was increased to 4,000*l.*, and finally other societies were associated with the Royal Society in its administration. The committee, however, was timorous, possibly owing to the apathy of the Universities and the general carelessness on such matters, and only one personal grant was made; the whole conception fell through.

Meantime, however, opinion has become more educated and alive to the extreme importance of research to the nation, and in 1891 a suggestion was made to the Royal Commission which administers the proceeds of the 1851 Exhibition that a sum of about 6,000*l.* a year available for scholarships should be employed in encouraging post-graduate research throughout the whole Empire. As what happened is told in the *Memoirs of Lord Playfair*, it is not indiscreet in me to state that when I proposed this new form of the endowment of research it would not have surprised me if the suggestion had been declined. It was carried through by Lord Playfair's

¹ In order to show how history is written, what actually happened on a fateful morning may be compared with the account given by Kinglake: 'Prince Louis rode home and went in out of sight. Then for the most part he remained close shut up in the Elysée. There, in an inner room, still decked in red trousers, but with his back to the daylight, they say he sat bent over a fireplace for hours and hours together, resting his elbows on his knees, and burying his face in his hands.'—*Crimean War*, vol. i. p. 245.

² See *Nature*, November and December, 1872.

enthusiastic support. This system has been at work ever since, and the good that has been done by it is now generally conceded.

It is a supreme satisfaction to me to know that in this present year of grace the national importance of the study of the *même inutile* is more generally recognised than it was during the times to which I have referred in my brief survey; and, indeed, we students are fortunate in having on our side in this matter two members of His Majesty's Government, who two years ago spoke with no uncertain sound upon this matter:—

‘Do we lack the imagination required to show what these apparently remote and abstract studies do for the happiness of mankind? We can appreciate that which obviously and directly ministers to human advancement and felicity, but seem, somehow or another, to be deficient in that higher form of imagination, in that longer sight, which sees in studies which have no obvious, necessary, or immediate result the foundation of the knowledge which shall give far greater happiness to mankind than any immediate, material, industrial advancement can possibly do; and I fear, and greatly fear, that, lacking that imagination, we have allowed ourselves to lag in the glorious race run now by civilised countries in pursuit of knowledge, and we have permitted ourselves so far to too large an extent to depend upon others for those additions to our knowledge which surely we might have made for ourselves.’¹

‘I would remind you that all history shows that progress—national progress of every kind—depends upon certain individuals rather than upon the mass. Whether you take religion, or literature, or political government, or art, or commerce, the new ideas, the great steps, have been made by individuals of superior quality and genius, who have, as it were, dragged the mass of the nation up one step to a higher level. So it must be in regard to material progress. The position of the nation to-day is due to the efforts of men like Watt and Arkwright, or, in our own time, to the Armstrongs, the Whitworths, the Kelvins, and the Siemenses. These are the men who, by their discoveries, by their remarkable genius, have produced the ideas upon which others have acted and which have permeated the whole mass of the nation and affected the whole of its proceedings. Therefore what we have to do, and this is our special task and object, is to produce more of these great men.’²

I finally come to the political importance of research. A country's research is as important in the long run as its battleships. The most eloquent teaching as to its national value we owe to Mr. Carnegie, for he has given the sum of 2,000,000*l.* to found a system of endowments, his chief purpose being, in his own words, ‘to secure if possible for the United States of America leadership in the domain of discovery and the utilisation of new forces for the benefit of man.’

¹ Mr. Balfour, *Nature*, May 30, 1901.

² Mr. Chamberlain, *Times*, January 18, 1901.

Here is a distinct challenge to Britain. Judging by experience in this country, in spite of the magnificent endowment of research by Mond and Lord Iveagh, the only source of possible competition in the British interest is the State, which certainly could not put the 1/8,000th part of the accumulated wealth of the country to better use ; for without such help both our Universities and our battleships will become of rapidly dwindling importance.

It is on this ground that I have included the importance of endowing research among the chief points to which I have been anxious to draw your attention.

The Need of a Scientific National Council.

In referring to the new struggle for existence among civilised communities I pointed out that the solution of a large number of scientific problems is now daily required for the State service, and that in this and other ways the source and standard of national efficiency have been greatly changed.

Much evidence bearing upon the amount of scientific knowledge required for the proper administration of the public departments, and the amount of scientific work done by and for the nation, was brought before the Royal Commission on Science presided over by the late Duke of Devonshire now more than a quarter of a century ago.

The Commission unanimously recommended that the State should be aided by a scientific council in facing the new problems constantly arising.

But while the home Government has apparently made up its mind to neglect the advice so seriously given, it should be a source of gratification to us all to know that the application of the resources of modern science to the economic, industrial, and agricultural development of India has for many years engaged the earnest attention of the Government of that country. The Famine Commissioners of 1878 laid much stress on the institution of scientific inquiry and experiment designed to lead to the gradual increase of the food-supply and to the greater stability of agricultural outturn, while the experience of recent years has indicated the increasing importance of the study of the economic products and mineral-bearing tracts.

Lord Curzon has recently ordered the heads of the various scientific departments to form a board, which shall meet twice annually, to begin with, to formulate a programme and to review past work. The board is also to act as an advisory committee to the Government,¹ providing among other matters for the proper co-ordination of all matters of scientific inquiry affecting India's welfare.

Lord Curzon is to be warmly congratulated upon the step he has taken, which is certain to bring benefit to our great Dependency.

¹ *Nature*, September 4, 1902.

The importance of such a board is many times greater at home, with so many external as well as internal interests to look after—problems common to peace and war, problems requiring the help of the economic as well as of the physical sciences.

It may be asked, What is done in Germany, where science is fostered and utilised far more than here ?

The answer is, There is such a council. I fancy, very much like what our Privy Council once was. It consists of representatives of the Ministry, the Universities, the industries, and agriculture. It is small, consisting of about a dozen members, consultative, and it reports direct to the Emperor. It does for industrial war what military and so-called defence councils do for national armaments ; it considers everything relating to the use of brain-power in peace—from alterations in school regulations and the organisation of the Universities, to railway rates and fiscal schemes, including the adjustment of duties. I am informed that what this council advises, generally becomes law.

It should be pretty obvious that a nation so provided must have enormous chances in its favour. It is a question of drilled battalions against an undisciplined army, of the use of the scientific spirit as opposed to the hope of ‘muddling through.’

Mr. Haldane has recently reminded us that ‘the weapons which science places in the hands of those who engage in great rivalries of commerce leave those who are without them, however brave, as badly off as were the dervishes of Omdurman against the maxims of Lord Kitchener.’

Without such a machinery as this, how can our Ministers and our rulers be kept completely informed on a thousand things of vital importance ? Why should our position and requirements as an industrial and thinking nation receive less attention from the authorities than the headdress of the Guards ? How, in the words of Lord Curzon,¹ can ‘the life and vigour of a nation be summed up before the world in the person of its sovereign’ if the national organisation is so defective that it has no means of keeping the head of the State informed on things touching the most vital and lasting interests of the country ? We seem to be still in the Palæolithic Age in such matters, the chief difference being that the sword has replaced the flint implement.

Some may say that it is contrary to our habit to expect the Government to interest itself too much or to spend money on matters relating to peace ; that war dangers are the only ones to be met or to be studied.

But this view leaves science and the progress of science out of the question. Every scientific advance is now, and will in the future be more and more, applied to war. It is no longer a question of an armed force with scientific corps ; it is a question of an armed force scientific

¹ *Times*, September 30, 1902.

from top to bottom. Thank God the Navy has already found this out. Science will ultimately rule all the operations both of peace and war, and therefore the industrial and the fighting population must both have a large common ground of education. Already it is not looking too far ahead to see that in a perfect State there will be a double use of each citizen—a peace use and a war use ; and the more science advances, the more the old difference between the peaceful citizen and the man at arms will disappear. The barrack, if it still exists, and the workshop will be assimilated ; the land unit, like the battleship, will become a school of applied science, self-contained, in which the officers will be the efficient teachers.

I do not think it is yet recognised how much the problem of national defence has thus become associated with that with which we are now chiefly concerned.

These, then, are some of the reasons which compel me to point out that a scientific council, which might be a scientific committee of the Privy Council, in dealing primarily with the national needs in times of peace, would be a source of strength to the nation.

To sum up, then. My earnest appeal to you is to gird up your loins and see to it that the science of the British Empire shall no longer remain unorganised. I have endeavoured to point out to you how the nation at present suffers from the absence of a powerful, continuous, reasoned expression of scientific opinion, urging in season and out of season that we shall be armed as other nations are, with efficient Universities and facilities for research to uphold the flag of Britain in the domain of learning and discovery, and what they alone can bring.

I have also endeavoured to show how, when this is done, the nation will still be less strong than it need be if there be not added to our many existing councils another, to secure that even during peace the benefits which a proper co-ordination of scientific effort in the nation's interest can bring shall not be neglected as they are at present.

Lest some of you may think that the scientific organisation which I trust you will determine to found would risk success in working on such large lines, let me remind you that in 1859, when the late Prince Consort occupied this Chair, he referred to 'impediments' to scientific progress, and said, 'they are often such as can only be successfully dealt with by the powerful arm of the State or the long purse of the nation.'

If the Prince Consort had lived to continue his advocacy of science, our position to-day would have been very different. His early death was as bad for Britain as the loss of a great campaign. If we cannot make up what we have lost, matters cannot mend.

I have done what I feel to be my duty in bringing the present condition of things before you. It is now your duty, if you agree with me, to see that it be put right. You can if you will.

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British Association for the Advancement of Science.

CAMBRIDGE, 1904.

ADDRESS

BY

THE RIGHT HON. A. J. BALFOUR, D.C.L., LL.D., M.P.,
F.R.S., Chancellor of the University of Edinburgh.

PRESIDENT.

Reflections suggested by the New Theory of Matter.

THE meetings of this great Society have for the most part been held in crowded centres of population, where our surroundings never permit us to forget, were such forgetfulness in any case possible, how close is the tie that binds modern science to modern industry, the abstract researches of the student to the labours of the inventor and the mechanic. This, no doubt, is as it should be. The interdependence of theory and practice cannot be ignored without inflicting injury on both ; and he is but a poor friend to either who undervalues their mutual co-operation.

Yet, after all, since the British Association exists for the advancement of science, it is well that now and again we should choose our place of gathering in some spot where science rather than its applications, knowledge, not utility, are the ends to which research is primarily directed.

If this be so, surely no happier selection could have been made than the quiet courts of this ancient University. For here, if anywhere, we tread the classic ground of physical discovery. Here, if anywhere, those who hold that physics is the true *Scientia Scientiarum*, the root of all the sciences which deal with inanimate nature, should feel themselves at home. For, unless I am led astray by too partial an affection for my own University, there is nowhere to be found, in any corner of the world, a spot with which have been connected, either by their training in youth, or by the labours of their maturer years, so many men eminent as the originators of new and fruitful physical conceptions. I say nothing of Bacon, the eloquent prophet of a new era ; nor of Darwin, the Copernicus of Biology ; for my subject to-day is not the contributions of Cambridge to the general growth of scientific knowledge. I am concerned rather with the illustrious line of physicists who have learned or taught within a few hundred yards of this building—a line stretching from Newton in the seventeenth century, through Cavendish in the eighteenth, through Young, Stokes, Maxwell, in the nineteenth, through Kelvin, who embodies an epoch in himself, down to Rayleigh, Larmor, J. J. Thomson, and the

scientific school centred in the Cavendish laboratory, whose physical speculations bid fair to render the closing years of the old century and the opening years of the new as notable as the greatest which have preceded them.

Now what is the task which these men, and their illustrious fellow-labourers out of all lands, have set themselves to accomplish? To what end led these 'new and fruitful physical conceptions' to which I have just referred? It is often described as the discovery of the 'laws connecting phenomena.' But this is certainly a misleading, and in my opinion a very inadequate, account of the subject. To begin with, it is not only inconvenient, but confusing, to describe as 'phenomena' things which do not appear, which never have appeared, and which never can appear, to beings so poorly provided as ourselves with the apparatus of sense perception. But apart from this, which is a linguistic error too deeply rooted to be easily exterminated, is it not most inaccurate in substance to say that a knowledge of Nature's laws is all we seek when investigating Nature? The physicist looks for something more than what, by any stretch of language, can be described as 'co-existences' and 'sequences' between so-called 'phenomena.' He seeks for something deeper than the laws connecting possible objects of experience. His object is physical reality: a reality which may or may not be capable of direct perception; a reality which is in any case independent of it; a reality which constitutes the permanent mechanism of that physical universe with which our immediate empirical connection is so slight and so deceptive. That such a reality exists, though philosophers have doubted, is the unalterable faith of science; and were that faith *per impossibile* to perish under the assaults of critical speculation, science, as men of science usually conceive it, would perish likewise.

If this be so, if one of the tasks of science, and more particularly of physics, is to frame a conception of the physical universe in its inner reality, then any attempt to compare the different modes in which, at different epochs of scientific development, this intellectual picture has been drawn, cannot fail to suggest questions of the deepest interest. True, I am precluded from dealing with such of these questions as are purely philosophical by the character of this occasion; and with such of them as are purely scientific by my own incompetence. But some there may be sufficiently near the dividing line to induce the specialists who rule by right on either side of it to view with forgiving eyes any trespasses into their legitimate domain which I may be tempted, during the next few minutes, to commit.

Let me, then, endeavour to compare the outlines of two such pictures, of which the first may be taken to represent the views prevalent towards the end of the eighteenth century; a little more than a hundred years from the publication of Newton's 'Principia,' and, roughly speaking, about midway between that epoch-making date and the present moment. I suppose that if at that period the average man of science had been asked to sketch his general conception of the physical universe, he would

probably have said that it essentially consisted of various sorts of ponderable matter, scattered in different combinations through space, exhibiting most varied aspects under the influence of chemical affinity and temperature, but through every metamorphosis obedient to the laws of motion, always retaining its mass unchanged, and exercising at all distances a force of attraction on other material masses, according to a simple law. To this ponderable matter he would (in spite of Rumford) have probably added the so-called 'imponderable' heat, then often ranked among the elements; together with the two 'electrical fluids,' and the corpuscular emanations supposed to constitute light.

In the universe as thus conceived, the most important forms of action between its constituents was action at a distance: the principle of the conservation of energy was, in any general form, undreamed of; electricity and magnetism, though already the subjects of important investigation, played no great part in the Whole of things; nor was a diffused ether required to complete the machinery of the universe.

Within a few months, however, of the date assigned for these deliverances of our hypothetical physicist, came an addition to this general conception of the world, destined profoundly to modify it. About a hundred years ago Young opened, or re-opened, the great controversy which finally established the undulatory theory of light, and with it a belief in an interstellar medium by which undulations could be conveyed. But this discovery involved much more than the substitution of a theory of light which was consistent with the facts for one which was not; since here was the first authentic introduction¹ into the scientific world-picture of a new and prodigious constituent—a constituent which has altered, and is still altering, the whole balance (so to speak) of the composition. Unending space, thinly strewn with suns and satellites, made or in the making, supplied sufficient material for the mechanism of the heavens as conceived by Laplace. Unending space filled with a continuous medium was a very different affair, and gave promise of strange developments. It could not be supposed that the ether, if its reality were once admitted, existed only to convey through interstellar regions the vibrations which happen to stimulate the optic nerve of man. Invented originally to fulfil this function, to this it could never be confined. And accordingly, as everyone now knows, things which, from the point of view of sense perception, are as distinct as light and radiant heat, and things to which sense perception makes no response, like the electric waves of wireless telegraphy,² intrinsically differ, not in kind, but in magnitude alone.

This, however, is not all, nor nearly all. If we jump over the century which separates 1804 from 1904, and attempt to give in outline the world-

¹ The hypothesis of an ether was, of course, not new. But before Young and Fresnel it cannot be said to have been established.

² First known through the theoretical work of Maxwell and the experiments of Herz.

picture as it now presents itself to some leaders of contemporary speculation, we shall find that in the interval it has been modified, not merely by such far-reaching discoveries as the atomic and molecular composition of ordinary matter, the kinetic theory of gases, and the laws of the conservation and dissipation of energy, but by the more and more important part which electricity and the ether occupy in any representation of ultimate physical reality.

Electricity was no more to the natural philosophers in the year 1700 than the hidden cause of an insignificant phenomenon.¹ It was known, and had long been known, that such things as amber and glass could be made to attract light objects brought into their neighbourhood; yet it was about fifty years before the effects of electricity were perceived in the thunderstorm. It was about 100 years before it was detected in the form of a current. It was about 120 years before it was connected with magnetism; about 170 years before it was connected with light and ethereal radiation.

But to-day there are those who regard gross matter, the matter of everyday experience, as the mere appearance of which electricity is the physical basis; who think that the elementary atom of the chemist, itself far beyond the limits of direct perception, is but a connected system of monads or sub-atoms which are not electrified matter, but are electricity itself; that these systems differ in the number of monads which they contain, in their arrangement, and in their motion relative to each other and to the ether; that on these differences, and on these differences alone, depend the various qualities of what have hitherto been regarded as indivisible and elementary atoms; and that while in most cases these atomic systems may maintain their equilibrium for periods which, compared with such astronomical processes as the cooling of a sun, may seem almost eternal, they are not less obedient to the law of change than the everlasting heavens themselves.

But if gross matter be a grouping of atoms, and if atoms be systems of electrical monads, what are these electrical monads? It may be that, as Professor Larmor has suggested, they are but a modification of the universal ether, a modification roughly comparable to a knot in a medium which is inextensible, incomprehensible and continuous. But whether this final unification be accepted or not, it is certain that these monads cannot be considered apart from the ether. It is on their interaction with the ether that their qualities depend; and without the ether an electric theory of matter is impossible.

Surely we have here a very extraordinary revolution. Two centuries ago electricity seemed but a scientific toy. It is now thought by many to constitute the reality of which matter is but the sensible expression. It is but a century ago that the title of an ether to a place among the

¹ The modern history of electricity begins with Gilbert, but I have throughout confined my observations to the post-Newtonian period.

constituents of the universe was authentically established. It seems possible now that it may be the stuff out of which that universe is wholly built. Nor are the collateral inferences associated with this view of the physical world less surprising. It used, for example, to be thought that mass was an original property of matter, neither capable of explanation nor requiring it ; in its nature essentially unchangeable, suffering neither augmentation nor diminution under the stress of any forces to which it could be subjected ; unalterably attached to, or identified with, each material fragment, howsoever much that fragment might vary in its appearance, its bulk, its chemical or its physical condition.

But if the new theories be accepted these views must be revised. Mass is not only explicable, it is actually explained. So far from being an attribute of matter considered in itself, it is due, as I have said, to the relation between the electrical monads of which matter is composed and the ether in which they are bathed. So far from being unchangeable, it changes, when moving at very high speeds, with every change in its velocity.

Perhaps, however, the most impressive alteration in our picture of the universe required by these new theories is to be sought in a different direction. We have all, I suppose, been interested in the generally accepted views as to the origin and development of suns with their dependent planetary systems ; and the gradual dissipation of the energy which during this process of concentration has largely taken the form of light and radiant heat. Follow out the theory to its obvious conclusions, and it becomes plain that the stars now visibly incandescent are those in mid-journey between the nebulae from which they sprang and the frozen darkness to which they are predestined. What, then, are we to think of the invisible multitude of the heavenly bodies in which this process has been already completed ? According to the ordinary view, we should suppose them to be in a state where all possibilities of internal movement were exhausted. At the temperature of interstellar space their constituent elements would be solid and inert ; chemical action and molecular movement would be alike impossible, and their exhausted energy could obtain no replenishment unless they were suddenly rejuvenated by some celestial collision, or travelled into other regions warmed by newer suns.

This view must, however, be profoundly modified if we accept the electric theory of matter. We can then no longer hold that if the internal energy of a sun were as far as possible converted into heat either by its contraction under the stress of gravitation or by chemical reactions between its elements, or by any other inter-atomic force ; and that, were the heat so generated to be dissipated, as in time it must be, through infinite space, its whole energy would be exhausted. On the contrary, the amount thus lost would be absolutely insignificant compared with what remained stored up within the separate atoms. The system in its corporate capacity would become bankrupt—the wealth of its individual

constituents would be scarcely diminished. They would lie side by side, without movement, without chemical affinity; yet each one, howsoever inert in its external relations, the theatre of violent motions, and of powerful internal forces.

Or, put the same thought in another form. When the sudden appearance of some new star in the telescopic field gives notice to the astronomer that he, and perhaps, in the whole universe, he alone, is witnessing the conflagration of a world, the tremendous forces by which this far-off tragedy is being accomplished must surely move his awe. Yet not only would the members of each separate atomic system pursue their relative course unchanged, while the atoms themselves were thus riven violently apart in flaming vapour, but the forces by which such a world is shattered are really *négligeable* compared with those by which each atom of it is held together.

In common, therefore, with all other living things, we seem to be practically concerned chiefly with the feebler forces of Nature, and with energy in its least powerful manifestations. Chemical affinity and cohesion are on this theory no more than the slight residual effects of the internal electrical forces which keep the atom in being. Gravitation, though it be the shaping force which concentrates nebulae into organised systems of suns and satellites, is trifling compared with the attractions and repulsions with which we are familiar between electrically charged bodies; while these again sink into insignificance beside the attractions and repulsions between the electric monads themselves. The irregular molecular movements which constitute heat, on which the very possibility of organic life seems absolutely to hang, and in whose transformations applied science is at present so largely concerned, cannot rival the kinetic energy stored within the molecules themselves. This prodigious mechanism seems outside the range of our immediate interests. We live, so to speak, merely on its fringe. It has for us no promise of utilitarian value. It will not drive our mills; we cannot harness it to our trains. Yet not less on that account does it stir the intellectual imagination. The starry heavens have from time immemorial moved the worship or the wonder of mankind. But if the dust beneath our feet be indeed compounded of innumerable systems, whose elements are ever in the most rapid motion, yet retain through uncounted ages their equilibrium unshaken, we can hardly deny that the marvels we directly see are not more worthy of admiration than those which recent discoveries have enabled us dimly to surmise.

Now, whether the main outlines of the world-picture which I have just imperfectly presented to you be destined to survive, or whether in their turn they are to be obliterated by some new drawing on the scientific palimpsest, all will, I think, admit that so bold an attempt to unify physical nature excites feelings of the most acute intellectual gratification. The satisfaction it gives is almost æsthetic in its intensity and quality.

We feel the same sort of pleasurable shock as when from the crest of some melancholy pass we first see far below us the sudden glories of plain, river, and mountain. Whether this vehement sentiment in favour of a simple universe has any theoretical justification I will not venture to pronounce. There is no *a priori* reason that I know of for expecting that the material world should be a modification of a single medium, rather than a composite structure built out of sixty or seventy elementary substances, eternal and eternally different. Why, then, should we feel content with the first hypothesis and not with the second? Yet so it is. Men of science have always been restive under the multiplication of entities. They have eagerly noted any sign that the chemical atom was composite, and that the different chemical elements had a common origin. Nor, for my part, do I think such instincts should be ignored. John Mill, if I rightly remember, was contemptuous of those who saw any difficulty in accepting the doctrine of 'action at a distance.' So far as observation and experiment can tell us, bodies *do* actually influence each other at a distance. And why should they not? Why seek to go behind experience in obedience to some *a priori* sentiment for which no argument can be adduced? So reasoned Mill, and to his reasoning I have no reply. Nevertheless, we cannot forget that it was to Faraday's obstinate disbelief in 'action at a distance' that we owe some of the crucial discoveries on which both our electric industries and the electric theory of matter are ultimately founded; while at this very moment physicists, however baffled in the quest for an explanation of gravity, refuse altogether to content themselves with the belief, so satisfying to Mill, that it is a simple and inexplicable property of masses acting on each other across space.

These obscure intimations about the nature of reality deserve, I think, more attention than has yet been given to them. That they exist is certain; that they modify the indifferent impartiality of pure empiricism can hardly be denied. The common notion that he who would search out the secrets of Nature must humbly wait on experience, obedient to its slightest hint, is but partly true. This may be his ordinary attitude; but now and again it happens that observation and experiment are not treated as guides to be meekly followed, but as witnesses to be broken down in cross-examination. Their plain message is disbelieved, and the investigating judge does not pause until a confession in harmony with his preconceived ideas has, if possible, been wrung from their reluctant evidence.

This proceeding needs neither explanation nor defence in those cases where there is an apparent contradiction between the utterances of experience in different connections. Such contradictions must of course be reconciled, and science cannot rest until the reconciliation is effected. The difficulty really arises when experience apparently says one thing and scientific instinct persists in saying another. Two such cases I have

already mentioned ; others will easily be found by those who care to seek. What is the origin of this instinct, and what its value ; whether it be a mere prejudice to be brushed aside, or a clue which no wise man would disdain to follow, I cannot now discuss. For other questions there are, not new, yet raised in an acute form by these most modern views of matter, on which I would ask your indulgent attention for yet a few moments.

That these new views diverge violently from those suggested by ordinary observation is plain enough. No scientific education is likely to make us, in our unreflective moments, regard the solid earth on which we stand, or the organised bodies with which our terrestrial fate is so intimately bound up, as consisting wholly of electric monads very sparsely scattered through the spaces which these fragments of matter are, by a violent metaphor, described as ‘occupying.’ Not less plain is it that an almost equal divergence is to be found between these new theories and that modification of the common-sense view of matter with which science has in the main been content to work.

What was this modification of common sense ? It is roughly indicated by an old philosophic distinction drawn between what were called the ‘primary’ and the ‘secondary’ qualities of matter. The primary qualities, such as shape and mass, were supposed to possess an existence quite independent of the observer ; and so far the theory agreed with common sense. The secondary qualities, on the other hand, such as warmth and colour, were thought to have no such independent existence, being, indeed, no more than the resultants due to the action of the primary qualities on our organs of sense-perception ; and here, no doubt, common sense and theory parted company.

You need not fear that I am going to drag you into the controversies with which this theory is historically connected. They have left abiding traces on more than one system of philosophy. They are not yet solved. In the course of them the very possibility of an independent physical universe has seemed to melt away under the solvent powers of critical analysis. But with all this I am not now concerned. I do not propose to ask what proof we have that an external world exists, or how, if it does exist, we are able to obtain cognisance of it. These may be questions very proper to be asked by philosophy ; but they are not proper questions to be asked by science. For, logically, they are antecedent to science, and we must reject the sceptical answers to both of them before physical science becomes possible at all. My present purpose requires me to do no more than observe that, be this theory of the primary and secondary qualities of matter good or bad, it is the one on which science has in the main proceeded. It was with matter thus conceived that Newton experimented. To it he applied his laws of motion ; of it he predicated universal gravitation. Nor was the case greatly altered when science became as much preoccupied with the movements of molecules as

it was with those of planets. For molecules and atoms, whatever else might be said of them, were at least pieces of matter, and, like other pieces of matter, possessed those 'primary' qualities supposed to be characteristic of all matter, whether found in large masses or in small.

But the electric theory which we have been considering carries us into a new region altogether. It does not confine itself to accounting for the secondary qualities by the primary, or the behaviour of matter in bulk by the behaviour of matter in atoms ; it analyses matter, whether molar or molecular, into something which is not matter at all. The atom is now no more than the relatively vast theatre of operations in which minute monads perform their orderly evolutions ; while the monads themselves are not regarded as units of matter, but as units of electricity ; so that matter is not merely explained, but is explained away.

Now the point to which I desire to call attention is not to be sought in the great divergence between matter as thus conceived by the physicist and matter as the ordinary man supposes himself to know it, between matter as it is perceived and matter as it really is, but to the fact that the first of these two quite inconsistent views is wholly based on the second.

This is surely something of a paradox. We claim to found all our scientific opinions on experience ; and the experience on which we found our theories of the physical universe is our *sense-perception* of that universe. That *is* experience ; and in this region of belief there is no other. Yet the conclusions which thus profess to be entirely founded upon experience are to all appearance fundamentally opposed to it ; our knowledge of reality is based upon illusion, and the very conceptions we use in describing it to others, or in thinking of it ourselves, are abstracted from anthropomorphic fancies, which science forbids us to believe and Nature compels us to employ.

We here touch the fringe of a series of problems with which inductive logic ought to deal, but which that most unsatisfactory branch of philosophy has systematically ignored. This is no fault of men of science. They are occupied in the task of making discoveries, not in that of analysing the fundamental presuppositions which the very possibility of making discoveries implies. Neither is it the fault of transcendental metaphysicians. Their speculations flourish on a different level of thought ; their interest in a philosophy of nature is lukewarm ; and howsoever the questions in which they are chiefly concerned be answered, it is by no means certain that the answers will leave the humbler difficulties at which I have hinted either nearer to or further from a solution. But though men of science and idealists stand acquitted, the same can hardly be said of empirical philosophers. So far from solving the problem, they seem scarcely to have understood that there was a problem to be solved. Led astray by a misconception to which I have already referred ; believing that science was concerned only with (so-called)

'phenomena,' that it had done all that it could be asked to do if it accounted for the sequence of our individual sensations, that it was concerned only with the 'laws of Nature,' and not with the inner character of physical reality; disbelieving, indeed, that any such physical reality does in truth exist;—it has never felt called upon seriously to consider what are the actual methods by which science attains its results, and how those methods are to be justified. If anyone, for example, will take up Mill's logic, with its 'sequences and co-existences between phenomena,' its 'method of difference,' its 'method of agreement,' and the rest; if he will then compare the actual doctrines of science with this version of the mode in which those doctrines have been arrived at,—he will soon be convinced of the exceedingly thin intellectual fare which has been hitherto served out to us under the imposing title of Inductive Theory.

There is an added emphasis given to these reflections by a train of thought which has long interested me, though I acknowledge that it never seems to have interested anyone else. Observe, then, that in order of logic sense-perceptions supply the premisses from which we draw all our knowledge of the physical world. It is they which tell us there is a physical world; it is on their authority that we learn its character. But in order of causation they are effects due (in part) to the constitution of our organs of sense. What we see depends not merely on what there is to be seen, but on our eyes. What we hear depends not merely on what there is to hear, but on our ears. Now, eyes and ears, and all the mechanism of perception, have, as we know, been evolved in us and our brute progenitors by the slow operation of Natural Selection. And what is true of sense-perception is of course also true of the intellectual powers which enable us to erect upon the frail and narrow platform which sense-perception provides, the proud fabric of the sciences.

Now Natural Selection only works through utility. It encourages aptitudes useful to their possessor or his species in the struggle for existence, and, for a similar reason, it is apt to discourage useless aptitudes, however interesting they may be from other points of view, because, being useless, they are probably burdensome.

But it is certain that our powers of sense-perception and of calculation were fully developed ages before they were effectively employed in searching out the secrets of physical reality—for our discoveries in this field are the triumphs but of yesterday. The blind forces of Natural Selection, which so admirably simulate design when they are providing for a present need, possess no power of prevision, and could never, except by accident, have endowed mankind, while in the making, with a physiological or mental outfit adapted to the higher physical investigations. So far as natural science can tell us, every quality of sense or intellect which does not help us to fight, to eat, and to bring up children, is but a by-product of the qualities which do. Our organs of sense-perception were not given us for purposes of research; nor was it to aid us in

meting out the heavens or dividing the atom that our powers of calculation and analysis were evolved from the rudimentary instincts of the animal.

It is presumably due to these circumstances that the beliefs of all mankind about the material surroundings in which it dwells are not only imperfect but fundamentally wrong. It may seem singular that down to, say, five years ago, our race has, without exception, lived and died in a world of illusions ; and that its illusions, or those with which we are here alone concerned, have not been about things remote or abstract, things transcendental or divine, but about what men see and handle, about those 'plain matters of fact' among which common sense daily moves with its most confident step and most self-satisfied smile. Presumably, however, this is either because too direct a vision of physical reality was a hindrance, not a help, in the struggle for existence ; because falsehood was more useful than truth ; or else because with so imperfect a material as living tissue no better results could be attained. But, if this conclusion be accepted, its consequences extend to other organs of knowledge besides those of perception. Not merely the senses, but the intellect, must be judged by it ; and it is hard to see why evolution, which has so lamentably failed to produce trustworthy instruments for obtaining the raw material of experience, should be credited with a larger measure of success in its provision of the physiological arrangements which condition reason in its endeavours to turn experience to account.

Considerations like these, unless I have compressed them beyond the limits of intelligibility, do undoubtedly suggest a certain inevitable incoherence in any general scheme of thought which is built out of materials provided by natural science alone. Extend the boundaries of knowledge as you may ; draw how you will the picture of the universe ; reduce its infinite variety to the modes of a single space-filling ether ; retrace its history to the birth of existing atoms ; show how under the pressure of gravitation they became concentrated into nebulae, into suns, and all the host of heaven ; how, at least in one small planet, they combined to form organic compounds ; how organic compounds became living things ; how living things, developing along many different lines, gave birth at last to one superior race ; how from this race arose, after many ages, a learned handful, who looked round on the world which thus blindly brought them into being, and judged it, and knew it for what it was :—perform, I say, all this, and, though you may indeed have attained to science, in nowise will you have attained to a self-sufficing system of beliefs. One thing at least will remain, of which this long-drawn sequence of causes and effects gives no satisfying explanation ; and that is knowledge itself. Natural science must ever regard knowledge as the product of irrational conditions, for in the last resort it knows no others. It must always regard knowledge as rational, or else science itself disappears. . In addition, therefore, to the difficulty of extracting from

experience beliefs which experience contradicts, we are confronted with the difficulty of harmonising the pedigree of our beliefs with their title to authority. The more successful we are in explaining their origin, the more doubt we cast on their validity. The more imposing seems the scheme of what we know, the more difficult it is to discover by what ultimate criteria we claim to know it.

Here, however, we touch the frontier beyond which physical science possesses no jurisdiction. If the obscure and difficult region which lies beyond is to be surveyed and made accessible, philosophy, not science, must undertake the task. It is no business of this Society. We meet here to promote the cause of knowledge in one of its great divisions ; we shall not help it by confusing the limits which usefully separate one division from another. It may perhaps be thought that I have disregarded my own precept—that I have wilfully overstepped the ample bounds within which the searchers into Nature carry on their labours. If it be so, I can only beg your forgiveness. My first desire has been to rouse in those who, like myself, are no specialists in physics, the same absorbing interest which I feel in what is surely the most far-reaching speculation about the physical universe which has ever claimed experimental support ; and if in so doing I have been tempted to hint my own personal opinion that as natural science grows it leans more, not less, upon an idealistic interpretation of the universe, even those who least agree may perhaps be prepared to pardon.

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British Association for the Advancement of Science.

SOUTH AFRICA, 1905.

ADDRESS

BY

PROFESSOR G. H. DARWIN, M.A., LL.D., PH.D., F.R.S.
PRESIDENT.

BARTHOLOMEU DIAZ, the discoverer of the Cape of Storms, spent sixteen months on his voyage, and the little flotilla of Vasco da Gama, sailing from Lisbon on July 8, 1497, only reached the Cape in the middle of November. These bold men, sailing in their puny fishing smacks to unknown lands, met the perils of the sea and the attacks of savages with equal courage. How great was the danger of such a voyage may be gathered from the fact that less than half the men who sailed with da Gama lived to return to Lisbon. Four hundred and eight years have passed since that voyage, and a ship of 13,000 tons has just brought us here, in safety and luxury, in but little more than a fortnight.

How striking are the contrasts presented by these events! On the one hand compare the courage, the endurance, and the persistence of the early navigators with the little that has been demanded of us; on the other hand consider how much man's power over the forces of Nature has been augmented during the past four centuries. The capacity for heroism is probably undiminished, but certainly the occasions are now rarer when it is demanded of us. If we are heroes, at least but few of us ever find it out, and, when we read stories of ancient feats of courage, it is hard to prevent an uneasy thought that, notwithstanding our boasted mechanical inventions, we are perhaps degenerate descendants of our great predecessors.

Yet the thought that to-day is less romantic and less heroic than yesterday has its consolation, for it means that the lot of man is easier than it was. Mankind, indeed, may be justly proud that this improvement has been due to the successive efforts of each generation to add to the heritage of knowledge handed down to it by its predecessors, whereby we have been born to the accumulated endowment of centuries of genius and labour.

I am told that in the United States the phrase 'I want to know' has

lost the simple meaning implied by the words, and has become a mere exclamation of surprise. Such a conventional expression could hardly have gained currency except amongst a people who aspire to knowledge. The dominance of the European race in America, Australasia, and South Africa has no doubt arisen from many causes, but amongst these perhaps the chief one is that not only do 'we want to know,' but also that we are determined to find out. And now within the last quarter of a century we have welcomed into the ranks of those who 'want to know' an oriental race, which has already proved itself strong in the peaceful arts of knowledge.

I take it, then, that you have invited us because you want to know what is worth knowing ; and we are here because we want to know you, to learn what you have to tell us, and to see that South Africa of which we have heard so much.

The hospitality which you are offering us is so lavish, and the journeys which you have organised are so extensive that the cynical observer might be tempted to describe our meeting as the largest picnic on record. Although we intend to enjoy our picnic with all our hearts, yet I should like to tell the cynic, if he is here, that perhaps the most important object of these conferences is the opportunity they afford for personal intercourse between men of like minds who live at the remotest corners of the earth.

We shall pass through your land with the speed and the voracity of a flight of locusts : but, unlike the locust, we shall, I hope, leave behind us permanent fertilisation in the form of stimulated scientific and educational activity. And this result will ensue whether or not we who have come from Europe are able worthily to sustain the lofty part of prophets of science. We shall try our best to play to your satisfaction on the great stage upon which you call on us to act, and if when we are gone you shall, amongst yourselves, pronounce the performance a poor one, yet the fact will remain, that this meeting has embodied in a material form the desire that the progress of this great continent shall not be merely material ; and such an aspiration secures its own fulfilment. However small may be the tangible results of our meeting, we shall always be proud to have been associated with you in your efforts for the advancement of science.

We do not know whether the last hundred years will be regarded for ever as the *seculum mirabile* of discovery, or whether it is but the prelude to yet more marvellous centuries. To us living men, who scarcely pass a year of our lives without witnessing some new marvel of discovery or invention, the rate at which the development of knowledge proceeds is truly astonishing ; but from a wider point of view the scale of time is relatively unimportant, for the universe is leisurely in its procedure. Whether the changes which we witness be fast or slow, they form a part of a long sequence of events which begin in some past of immeasurable remoteness and tend to some end which we cannot fore-

see. It must always be profoundly interesting to the mind of man to trace successive cause and effect in the chain of events which make up the history of the earth and all that lives on it, and to speculate on the origin and future fate of animals, and of planets, suns, and stars. I shall try, then, to set forth in my address some of the attempts which have been made to formulate Evolutionary speculation. This choice of a subject has moreover been almost forced on me by the scope of my own scientific work, and it is, I think, justified by the name which I bear. It will be my fault and your misfortune if I fail to convey to you some part of the interest which is naturally inherent in such researches.

The man who propounds a theory of evolution is attempting to reconstruct the history of the past by means of the circumstantial evidence afforded by the present. The historian of man, on the other hand, has the advantage over the evolutionist in that he has the written records of the past on which to rely. The discrimination of the truth from amongst discordant records is frequently a work demanding the highest qualities of judgment; yet when this end is attained it remains for the historian to convert the arid skeleton of facts into a living whole by clothing it with the flesh of human motives and impulses. For this part of his task he needs much of that power of entering into the spirit of other men's lives which goes to the making of a poet. Thus the historian should possess not only the patience of the man of science in the analysis of facts, but also the imagination of the poet to grasp what the facts have meant. Such a combination is rarely to be found in equal perfection on both sides, and it would not be hard to analyse the works of great historians so as to see which quality was predominant in each of them.

The evolutionist is spared the surpassing difficulty of the human element, yet he also needs imagination, although of a different character from that of the historian. In its lowest form his imagination is that of the detective who reconstructs the story of a crime; in its highest it demands the power of breaking loose from all the trammels of convention and education, and of imagining something which has never occurred to the mind of man before. In every case the evolutionist must form a theory for the facts before him, and the great theorist is only to be distinguished from the fantastic fool by the sobriety of his judgment—a distinction, however, sufficient to make one rare and the other only too common.

The test of a scientific theory lies in the number of facts which it groups into a connected whole; it ought besides to be fruitful in pointing the way to the discovery and co-ordination of new and previously unsuspected facts. Thus a good theory is in effect a cyclopædia of knowledge, susceptible of indefinite extension by the addition of supplementary volumes.

Hardly any theory is all true, and many are not all false. A theory may be essentially at fault and yet point the way to truth

and so justify its temporary existence. We should not, therefore, totally reject one or other of two rival theories on the ground that they seem, with our present knowledge, mutually inconsistent, for it is likely that both may contain important elements of truth. The theories of which I shall have to speak hereafter may often appear discordant with one another according to our present lights. Yet we must not scruple to pursue the several divergent lines of thought to their logical conclusions, relying on future discovery to eliminate the false and to reconcile together the truths which form part of each of them.

In the mouths of the unscientific evolution is often spoken of as almost synonymous with the evolution of the various species of animals on the earth, and this again is sometimes thought to be practically the same thing as the theory of Natural Selection. Of course those who are conversant with the history of scientific ideas are aware that a belief in the gradual and orderly transformation of Nature, both animate and inanimate, is of great antiquity.

We may liken the facts on which theories of evolution are based to a confused heap of beads, from which a keen-sighted searcher after truth picks out and strings together a few which happen to catch his eye, as possessing certain resemblances. Until recently, theories of evolution in both realms of Nature were partial and discontinuous, and the chains of facts were correspondingly short and disconnected. At length the theory of Natural Selection, by formulating the cause of the divergence of forms in the organic world from the parental stock, furnished the naturalist with a clue by which he examined the disordered mass of facts before him, and he was thus enabled to go far in deducing order where chaos had ruled before, but the problem of reducing the heap to perfect order will probably baffle the ingenuity of the investigator for ever.

So illuminating has been this new idea that, as the whole of Nature has gradually been re-examined by its aid, thousands of new facts have been brought to light, and have been strung in due order on the necklace of knowledge. Indeed the transformation resulting from the new point of view has been so far-reaching as almost to justify the misapprehension of the unscientific as to the date when the doctrines of evolution first originated in the mind of man.

It is not my object, nor indeed am I competent, to examine the extent to which the Theory of Natural Selection has needed modification since it was first formulated by my father and Wallace. But I am surely justified in maintaining that the general principle holds its place firmly as a permanent acquisition to modes of thought.

Evolutionary doctrines concerning inanimate nature, although of much older date than those which concern life, have been profoundly affected by the great impulse of which I have spoken. It has thus come about that the origin and history of the chemical elements and of stellar systems now occupy a far larger space in the scientific mind than was formerly the case. The subject which I shall discuss to-night is the extent to which

ideas, parallel to those which have done so much towards elucidating the problems of life, hold good also in the world of matter ; and I believe that it will be possible to show that in this respect there exists a resemblance between the two realms of nature, which is not merely fanciful. It is proper to add that as long ago as 1873 Baron Karl du Prel discussed the same subject from a similar point of view, in a book entitled 'The Struggle for Life in the Heavens.'¹

Although inanimate matter moves under the action of forces which are incomparably simpler than those governing living beings, yet the problems of the physicist and the astronomer are scarcely less complex than those which present themselves to the biologist. The mystery of life remains as impenetrable as ever, and in his evolutionary speculations the biologist does not attempt to explain life itself, but, adopting as his unit the animal as a whole, discusses its relationships to other animals and to the surrounding conditions. The physicist, on the other hand, is irresistibly impelled to form theories as to the intimate constitution of the ultimate parts of matter, and he desires further to piece together the past histories and the future fates of planets, stars, and nebulae. If then the speculations of the physicist seem in some respects less advanced than those of the biologist, it is chiefly because he is more ambitious in his aims. Physicists and astronomers have not yet found their Johannesburg or Kimberley ; but although we are still mere prospectors, I am proposing to show you some of the dust and diamonds which we have already extracted from our surface mines.

The fundamental idea in the theory of Natural Selection is the persistence of those types of life which are adapted to their surrounding conditions, and the elimination by extermination of ill-adapted types. The struggle for life amongst forms possessing a greater or less degree of adaptation to slowly varying conditions is held to explain the gradual transmutation of species. Although a different phraseology is used when we speak of the physical world, yet the idea is essentially the same.

The point of view from which I wish you to consider the phenomena of the world of matter may be best explained if, in the first instance, I refer to political institutions, because we all understand, or fancy we understand, something of politics, whilst the problems of physics are commonly far less familiar to us. This illustration will have a further advantage in that it will not be a mere parable, but will involve the fundamental conception of the nature of evolution.

The complex interactions of man with man in a community are usually described by such comprehensive terms as the State, the Commonwealth, or the Government. Various States differ widely in their constitution and in the degree of the complexity of their organisation, and we classify them by various general terms, such as autocracy, aristocracy, or democracy, which express somewhat loosely their leading characteristics.

¹ *Der Kampf um's Dasein am Himmel* (zweite Auflage), Denicke, Berlin, 1876.

But, for the purpose of showing the analogy with physics, we need terms of wider import than those habitually used in politics. All forms of the State imply inter-relationship in the actions of men, and action implies movement. Thus the State may be described as a configuration or arrangement of a community of men ; or we may say that it implies a definite mode of motion of man—that is to say an organised scheme of action of man on man. Political history gives an account of the gradual changes in such configurations or modes of motion of men as have possessed the quality of persistence or of stability to resist the disintegrating influence of surrounding circumstances.

In the world of life the naturalist describes those forms which persist as species ; similarly the physicist speaks of stable configurations or modes of motion of matter ; and the politician speaks of States. The idea at the base of all these conceptions is that of stability, or the power of resisting disintegration. In other words, the degree of persistence or permanence of a species, of a configuration of matter, or of a State depends on the perfection of its adaptation to its surrounding conditions.

If we trace the history of a State we find the degree of its stability gradually changing, slowly rising to a maximum, and then slowly declining. When it falls to nothing a revolution ensues, and a new form of government is established. The new mode of motion or government has at first but slight stability, but it gradually acquires strength and permanence, until in its turn the slow decay of stability leads on to a new revolution.

Such crises in political history may give rise to a condition in which the State is incapable of perpetuation by transformation. This occurs when a savage tribe nearly exterminates another tribe and leads the few survivors into slavery ; the previous form of government then becomes extinct.

The physicist, like the biologist and the historian, watches the effect of slowly varying external conditions ; he sees the quality of persistence or stability gradually decaying until it vanishes, when there ensues what is called, in politics, a revolution.

These considerations lead me to express a doubt whether biologists have been correct in looking for continuous transformation of species. Judging by analogy we should rather expect to find slight continuous changes occurring during a long period of time, followed by a somewhat sudden transformation into a new species, or by rapid extinction. However this may be, when the stability of a mode of motion vanishes, the physicist either finds that it is replaced by a new persistent type of motion adapted to the changed conditions, or perhaps that no such transformation is possible and that the mode of motion has become extinct. The evanescent type of animal life has often been preserved for us, fossilised in geological strata ; the evanescent form of government is preserved in written records or in the customs of savage tribes ; but the physicist has to pursue his investigations without such useful hints as to the past.

The time-scale in the transmutation of species of animals is furnished by the geological record, although it is not possible to translate that record into years. As we shall see hereafter, the time needed for a change of type in atoms or molecules may be measured by millionths of a second, while in the history of the stars continuous changes may occupy millions of years. Notwithstanding this gigantic contrast in speed, yet the process involved seems to be essentially the same.

It is hardly too much to assert that, if the conditions which determine stability of motion could be accurately formulated throughout the universe, the past history of the cosmos and its future fate would be unfolded. How indefinitely far we stand removed from such a state of knowledge will become abundantly clear from the remainder of my address.

The study of stability and instability then furnishes the problems which the physicist and biologist alike attempt to solve. The two classes of problems differ principally in the fact that the conditions of the world of life are so incomparably more intricate than those of the world of matter that the biologist is compelled to abandon the attempt to determine the absolute amount of the influence of the various causes which have affected the existence of species. His conclusions are merely qualitative and general, and he is almost universally compelled to refrain from asserting even in general terms what are the reasons which have rendered one form of animal life stable and persistent, and another unstable and evanescent.

On the other hand, the physicist, as a general rule, does not rest satisfied unless he obtains a quantitative estimate of various causes and effects on the systems of matter which he discusses. Yet there are some problems of physical evolution in which the conditions are so complex that the physicist is driven, as is the biologist, to rest satisfied with qualitative rather than quantitative conclusions. But he is not content with such crude conclusions except in the last resort, and he generally prefers to proceed by a different method.

The mathematician mentally constructs an ideal mechanical system or model, which is intended to represent in its leading features the system he wants to examine. It is often a task of the utmost difficulty to devise such a model, and the investigator may perchance unconsciously drop out as unimportant something which is really essential to represent actuality. He next examines the conditions of his ideal system, and determines, if he can, all the possible stable and unstable configurations, together with the circumstances which will cause transitions from one to the other. Even when the working model has been successfully imagined, this latter task may often overtax the powers of the mathematician. Finally it remains for him to apply his results to actual matter, and to form a judgment of the extent to which it is justifiable to interpret Nature by means of his results.

The remainder of my address will be occupied by an account of

various investigations which will illustrate the principles and methods which I have now explained in general terms.

The fascinating idea that matter of all kinds has a common substratum is of remote antiquity. In the Middle Ages the alchemists, inspired by this idea, conceived the possibility of transforming the baser metals into gold. The sole difficulty seemed to them the discovery of an appropriate series of chemical operations. We now know that they were always indefinitely far from the goal of their search, yet we must accord to them the honour of having been the pioneers of modern chemistry.

The object of alchemy, as stated in modern language, was to break up or dissociate the atoms of one chemical element into its component parts, and afterwards to reunite them into atoms of gold. Although even the dissociative stage of the alchemistic problem still lies far beyond the power of the chemist, yet modern researches seem to furnish a sufficiently clear idea of the structure of atoms to enable us to see what would have to be done to effect a transformation of elements. Indeed, in the complex changes which are found to occur spontaneously in uranium, radium, and the allied metals we are probably watching a spontaneous dissociation and transmutation of elements.

Natural Selection may seem, at first sight, as remote as the poles asunder from the ideas of the alchemist, yet dissociation and transmutation depend on the instability and regained stability of the atom, and the survival of the stable atom depends on the principle of Natural Selection.

Until some ten years ago the essential diversity of the chemical elements was accepted by the chemist as an ultimate fact, and indeed the very name of atom, or that which cannot be cut, was given to what was supposed to be the final indivisible portion of matter. The chemist thus proceeded in much the same way as the biologist who, in discussing evolution, accepts the species as his working unit. Accordingly, until recently the chemist discussed working models of matter of atomic structure, and the vast edifice of modern chemistry has been built with atomic bricks.

But within the last few years the electrical researches of Lenard, Röntgen, Becquerel, the Curies, of my colleagues Larmor and Thomson, and of a host of others, have shown that the atom is not indivisible, and a flood of light has been thrown thereby on the ultimate constitution of matter. Amongst all these fertile investigators it seems to me that Thomson stands pre-eminent, because it is principally through him that we are to-day in a better position for picturing the structure of an atom than was ever the case before.

Even if I had the knowledge requisite for a complete exposition of these investigations, the limits of time would compel me to confine myself

to those parts of the subject which bear on the constitution and origin of the elements.

It has been shown, then, that the atom, previously supposed to be indivisible, really consists of a large number of component parts. By various convergent lines of experiment it has been proved that the simplest of all atoms, namely that of hydrogen, consists of about 800 separate parts; while the number of parts in the atom of the denser metals must be counted by tens of thousands. These separate parts of the atom have been called corpuscles or electrons, and may be described as particles of negative electricity. It is paradoxical, yet true, that the physicist knows more about these ultra-atomic corpuscles and can more easily count them than is the case with the atoms of which they form the parts.

The corpuscles, being negatively electrified, repel one another just as the hairs on a person's head mutually repel one another when combed with a vulcanite comb. The mechanism is as yet obscure whereby the mutual repulsion of the negative corpuscles is restrained from breaking up the atom, but a positive electrical charge, or something equivalent thereto, must exist in the atom, so as to prevent disruption. The existence in the atom of this community of negative corpuscles is certain, and we know further that they are moving with speeds which may in some cases be comparable to the velocity of light, namely, 200,000 miles a second. But the mechanism whereby they are held together in a group is hypothetical.

It is only just a year ago that Thomson suggested, as representing the atom, a mechanical or electrical model whose properties could be accurately examined by mathematical methods. He would be the first to admit that his model is at most merely a crude representation of actuality, yet he has been able to show that such an atom must possess mechanical and electrical properties which simulate, with what Whetham describes as 'almost Satanic exactness,' some of the most obscure and yet most fundamental properties of the chemical elements. 'Se non è vero, è ben trovato,' and we are surely justified in believing that we have the clue which the alchemists sought in vain.

Thomson's atom consists of a globe charged with positive electricity, inside which there are some thousand or thousands of corpuscles of negative electricity, revolving in regular orbits with great velocities. Since two electrical charges repel one another if they are of the same kind, and attract one another if they are of opposite kinds, the corpuscles mutually repel one another, but all are attracted by the globe containing them. The forces called into play by these electrical interactions are clearly very complicated, and you will not be surprised to learn that Thomson found himself compelled to limit his detailed examination of the model atom to one containing about seventy corpuscles. It is indeed a triumph of mathematical power to have determined the mechanical conditions of such a miniature planetary system as I have described.

It appears that in general there are definite arrangements of the orbits in which the corpuscles must revolve, if they are to be persistent or stable in their motions. But the number of corpuscles in such a community is not absolutely fixed. It is easy to see that we might add a minor planet, or indeed half a dozen minor planets, to the solar system without any material derangement of the whole; but it would not be possible to add a hundred planets with an aggregate mass equal to that of Jupiter without derangement of the solar system. So also we might add or subtract from an atom three or four corpuscles from a system containing a thousand corpuscles moving in regular orbits without any profound derangement. As each arrangement of orbits corresponds to the atom of a distinct element, we may say that the addition or subtraction of a few corpuscles to the atom will not effect a transmutation of elements. An atom which has a deficiency of its full complement of corpuscles, which it will be remembered are negative, will be positively electrified, while one with an excess of corpuscles will be negatively electrified. I have referred to the possibility of a deficiency or excess of corpuscles because it is important in Thomson's theory; but, as it is not involved in the point of view which I wish to take, I will henceforth only refer to the normal or average number in any arrangement of corpuscles. Accordingly we may state that definite numbers of corpuscles are capable of association in stable communities of definite types.

An infinite number of communities are possible, possessing greater or lesser degrees of stability. Thus the corpuscles in one such community might make thousands of revolutions in their orbits before instability declared itself; such an atom might perhaps last for a long time as estimated in millionths of seconds, but it must finally break up and the corpuscles must disperse or rearrange themselves after the ejection of some of their number. We are thus led to conjecture that the several chemical elements represent those different kinds of communities of corpuscles which have proved by their stability to be successful in the struggle for life. If this is so, it is almost impossible to believe that the successful species have existed for all time, and we must hold that they originated under conditions about which I must forbear to follow Sir Norman Lockyer in speculating.¹

But if the elements were not eternal in the past, we must ask whether there is reason to believe that they will be eternal in the future. Now, although the conception of the decay of an element and its spontaneous transmutation into another element would have seemed absolutely repugnant to the chemist until recently, yet analogy with other moving systems seems to suggest that the elements are not eternal.

At any rate it is of interest to pursue to its end the history of the model atom which has proved to be so successful in imitating the properties of matter. The laws which govern electricity in motion indicate

¹ *Inorganic Evolution*, Macmillan, 1900.

that such an atom must be radiating or losing energy, and therefore a time must come when it will run down, as a clock does. When this time comes it will spontaneously transmute itself into an element which needs less energy than was required in the former state. Thomson conceives that an atom might be constructed after his model so that its decay should be very slow. It might, he thinks, be made to run for a million years, but it would not be eternal.

Such a conclusion is an absolute contradiction to all that was known of the elements until recently, for no symptoms of decay are perceived, and the elements existing in the solar system must already have lasted for millions of years. Nevertheless, there is good reason to believe that in radium, and in other elements possessing very complex atoms, we do actually observe that break-up and spontaneous rearrangement which constitute a transmutation of elements.

It is impossible as yet to say how science will solve this difficulty, but future discovery in this field must surely prove deeply interesting. It may well be that the train of thought which I have sketched will ultimately profoundly affect the material side of human life, however remote it may now seem from our experiences of daily life.

I have not as yet made any attempt to represent the excessive minuteness of the corpuscles, of whose existence we are now so confident; but, as an introduction to what I have to speak of next, it is necessary to do so. To obtain any adequate conception of their size we must betake ourselves to a scheme of threefold magnification. Lord Kelvin has shown that, if a drop of water were magnified to the size of the earth, the molecules of water would be of a size intermediate between that of a cricket-ball and of a marble. Now each molecule contains three atoms, two being of hydrogen and one of oxygen. The molecular system probably presents some sort of analogy with that of a triple star; the three atoms, replacing the stars, revolving about one another in some sort of dance which cannot be exactly described. I doubt whether it is possible to say how large a part of the space occupied by the whole molecule is occupied by the atoms; but perhaps the atoms bear to the molecule some such relationship as the molecule to the drop of water referred to. Finally, the corpuscles may stand to the atom in a similar scale of magnitude. Accordingly a threefold magnification would be needed to bring these ultimate parts of the atom within the range of our ordinary scales of measurement.

I have already considered what would be observed under the triply powerful microscope, and must now return to the intermediate stage of magnification, in which we consider those communities of atoms which form molecules. This is the field of research of the chemist. Although prudence would tell me that it would be wiser not to speak of a subject of which I know so little, yet I cannot refrain from saying a few words.

The community of atoms in water has been compared with a triple star, but there are others known to the chemist in which the atoms are

to be counted by fifties and hundreds, so that they resemble constellations.

I conceive that here again we meet with conditions similar to those which we have supposed to exist in the atom. Communities of atoms are called chemical combinations, and we know that they possess every degree of stability. The existence of some is so precarious that the chemist in his laboratory can barely retain them for a moment ; others are so stubborn that he can barely break them up. In this case dissociation and reunion into new forms of communities are in incessant and spontaneous progress throughout the world. The more persistent or more stable combinations succeed in their struggle for life, and are found in vast quantities, as in the cases of common salt and of the combinations of silicon. But no one has ever found a mine of gun-cotton, because it has so slight a power of resistance. If, through some accidental collocation of elements, a single molecule of gun-cotton were formed, it would have but a short life.

Stability is, further, a property of relationship to surrounding conditions ; it denotes adaptation to environment. Thus salt is adapted to the struggle for existence on the earth, but it cannot withstand the severer conditions which exist in the sun.

Thus far we have been concerned with the almost inconceivably minute, and I now propose to show that similar conditions prevail on a larger scale.

Many geological problems might well be discussed from my present point of view, yet I shall pass them by, and shall proceed at once to Astronomy, beginning with the smallest cosmical scale of magnitude, and considering afterwards the larger celestial phenomena.

The problems of cosmical evolution are so complicated that it is well to conduct the attack in various ways at the same time. Although the several theories may seem to some extent discordant with one another, yet, as I have already said, we ought not to scruple to carry each to its logical conclusion. We may be confident that in time the false will be eliminated from each theory, and when the true alone remains the reconciliation of apparent disagreements will have become obvious.

The German astronomer Bode long ago propounded a simple empirical law concerning the distances at which the several planets move about the sun. It is true that the planet Neptune, discovered subsequently, was found to be considerably out of the place which would be assigned to it by Bode's law, yet his formula embraces so large a number of cases with accuracy that we are compelled to believe that it arises in some manner from the primitive conditions of the planetary system.

The explanation of the causes which have led to this simple law as to the planetary distances presents an interesting problem, and, although it is still unsolved, we may obtain some insight into its meaning by considering what I have called a working model of ideal simplicity.

Imagine then a sun round which there moves in a circle a single large planet. I will call this planet Jove, because it may be taken as a representative of our largest planet, Jupiter. Suppose next that a meteoric stone or small planet is projected in any perfectly arbitrary manner in the same plane in which Jove is moving; then we ask how this third body will move. The conditions imposed may seem simple, yet the problem has so far overtaxed the powers of the mathematician that nothing approaching a general answer to our question has yet been given. We know, however, that under the combined attractions of the sun and Jove the meteoric stone will in general describe an orbit of extraordinary complexity, at one time moving slowly at a great distance from both the sun and Jove, at other times rushing close past one or other of them. As it grazes past Jove or the sun it may often but just escape a catastrophe, but a time will come at length when it runs its chances too fine and comes into actual collision. The individual career of the stone is then ended by absorption, and of course by far the greater chance is that it will find its Nirvana by absorption in the sun.

Next let us suppose that instead of one wandering meteoric stone or minor planet there are hundreds of them, moving initially in all conceivable directions. Since they are all supposed to be very small, their mutual attractions will be insignificant, and they will each move almost as though they were influenced only by the sun and Jove. Most of these stones will be absorbed by the sun, and the minority will collide with Jove.

When we inquire how long the career of a stone may be, we find that it depends on the direction and speed with which it is started, and that by proper adjustment the delay of the final catastrophe may be made as long as we please. Thus by making the delay indefinitely long we reach the conception of a meteoric stone which moves so as never to come into collision with either body.

There are, therefore, certain perpetual orbits in which a meteoric stone or minor planet may move for ever without collision. But when such an immortal career has been discovered for our minor planet, it still remains to discover whether the slightest possible departure from the prescribed orbit will become greater and greater and ultimately lead to a collision with the sun or Jove, or whether the body will travel so as to cross and recross the exact perpetual orbit, always remaining close to it. If the slightest departure inevitably increases as time goes on, the orbit is unstable; if, on the other hand, it only leads to a slight waviness in the path described, it is stable.

We thus arrive at another distinction: there are perpetual orbits, but some, and indeed most, are unstable, and these do not offer an immortal career for a meteoric stone; and there are other perpetual orbits which are stable or persistent. The unstable ones are those which succumb in the struggle for life, and the stable ones are the species adapted to their environment.

If, then, we are given a system of a sun and large planet, together

with a swarm of small bodies moving in all sorts of ways, the sun and planet will grow by accretion, gradually sweeping up the dust and rubbish of the system, and there will survive a number of small planets and satellites moving in certain definite paths. The final outcome will be an orderly planetary system in which the various orbits are arranged according to some definite law.

But the problem presented even by a system of such ideal simplicity is still far from having received a complete solution. No general plan for determining perpetual orbits has yet been discovered, and the task of discriminating the stable from the unstable is arduous. But a beginning has been made in the determination of some of the zones surrounding the sun and Jove in which stable orbits are possible, and others in which they are impossible. There is hardly room for doubt that if a complete solution for our solar system were attainable, we should find that the orbits of the existing planets and satellites are numbered amongst the stable perpetual orbits, and should thus obtain a rigorous mechanical explanation of Bode's law concerning the planetary distances.

It is impossible not to be struck by the general similarity between the problem presented by the corpuscles moving in orbits in the atom, and that of the planets and satellites moving in a planetary system. It may not, perhaps, be fanciful to imagine that some general mathematical method devised for solving a problem of cosmical evolution may find another application to miniature atomic systems, and may thus lead onward to vast developments of industrial mechanics. Science, however diverse its aims, is a whole, and men of science do well to impress on the captains of industry that they should not look askance on those branches of investigation which may seem for the moment far beyond any possibility of practical utility.

You will remember that I discussed the question as to whether the atomic communities of corpuscles could be regarded as absolutely eternal, and that I said that the analogy of other moving systems pointed to their ultimate mortality. Now the chief analogy which I had in my mind was that of a planetary system.

The orbits of which I have spoken are only perpetual when the bodies are infinitesimal in mass, and meet with no resistance as they move. Now the infinitesimal body does not exist, and both Lord Kelvin and Poincaré concur in holding that disturbance will ultimately creep in to any system of bodies moving even in so-called stable orbits; and this is so even apart from the resistance offered to the moving bodies by any residual gas there may be scattered through space. The stability is therefore only relative, and a planetary system contains the seeds of its own destruction. But this ultimate fate need not disturb us either practically or theoretically, for the solar system contains in itself other seeds of decay which will probably bear fruit long before the occurrence of any serious disturbance of the kind of which I speak.

Before passing on to a new topic I wish to pay a tribute to the men

to whom we owe the recent great advances in theoretical dynamical astronomy. As treated by the master-hands of Lagrange and Laplace and their successors, this branch of science hardly seemed to afford scope for any great new departure. But that there is always room for discovery, even in the most frequented paths of knowledge, was illustrated when, nearly thirty years ago, Hill of Washington proposed a new method of treating the theory of the moon's motion in a series of papers which have become classical. I have not time to speak of the enormous labour and great skill involved in the completion of Hill's Lunar Theory, by Ernest Brown, whom I am glad to number amongst my pupils and friends ; for I must confine myself to other aspects of Hill's work.

The title of Hill's most fundamental paper, namely, 'On Part of the Motion of the Lunar Perigee,' is almost comic in its modesty, for who would suspect that it contains the essential points involved in the determination of perpetual orbits and their stability ? Probably Hill himself did not fully realise at the time the full importance of what he had done. Fortunately he was followed by Poincaré, who not only saw its full meaning but devoted his incomparable mathematical powers to the full theoretical development of the point of view I have been laying before you.

Other mathematicians have also made contributions to this line of investigation, amongst whom I may number my friend Mr. Hough, chief assistant at the Royal Observatory of Cape Town, and myself. But without the work of our two great forerunners we should still be in utter darkness, and it would have been impossible to give even this slight sketch of a great subject.

The theory which I have now explained points to the origin of the sun and planets from gradual accretions of meteoric stones, and it makes no claim to carry the story back behind the time when there was already a central condensation or sun about which there circled another condensation or planet. But more than a century ago an attempt had already been made to reconstruct the history back to a yet remoter past, and, as we shall see, this attempt was based upon quite a different supposition as to the constitution of the primitive solar system. I myself believe that the theory I have just explained, as well as that to which I am coming, contains essential elements of truth, and that the apparent discordances will some day be reconciled. The theory of which I speak is the celebrated Nebular Hypothesis, first suggested by the German philosopher Kant, and later restated independently and in better form by the French mathematician Laplace.

Laplace traced the origin of the solar system to a nebula or cloud of rarefied gas congregated round a central condensation which was ultimately to form the sun. The whole was slowly rotating about an axis through its centre, and, under the combined influences of rotation and of the mutual attraction of the gas, it assumed a globular form, slightly flattened at the poles. The justifiability of this supposition is confirmed

by the observations of astronomers, for they find in the heavens many nebulae, while the spectroscope proves that their light at any rate is derived from gas. The primeval globular nebula is undoubtedly a stable or persistent figure, and thus Laplace's hypothesis conforms to the general laws which I have attempted to lay down.

The nebula must have gradually cooled by radiation into space, and as it did so the gas must necessarily have lost some of its spring or elasticity. This loss of power of resistance then permitted the gas to crowd more closely towards the central condensation, so that the nebula contracted. The contraction led to two results, both inevitable according to the laws of mechanics : first, the central condensation became hotter ; and, secondly, the speed of its rotation became faster. The accelerated rotation led to an increase in the amount of polar flattening, and the nebula at length assumed the form of a lens, or of a disk thicker in the middle than at the edges. Assuming the existence of the primitive nebula, the hypothesis may be accepted thus far as practically certain.

From this point, however, doubt and difficulty enter into the argument. It is supposed that the nebula became so much flattened that it could not subsist as a continuous aggregation of gas, and a ring of matter detached itself from the equatorial regions. The central portions of the nebula, when relieved of the excrescence, resumed the more rounded shape formerly possessed by the whole. As the cooling continued the central portion in its turn became excessively flattened through the influence of its increased rotation ; another equatorial ring then detached itself, and the whole process was repeated as before. In this way the whole nebula was fissured into a number of rings surrounding the central condensation, whose temperature must by then have reached incandescence.

Each ring then aggregated itself round some nucleus which happened to exist in its circumference, and so formed a subordinate nebula. Passing through a series of transformations, like its parent, this nebula was finally replaced by a planet with attendant satellites.

The whole process forms a majestic picture of the history of our system. But the mechanical conditions of a rotating nebula are too complex to admit, as yet, of complete mathematical treatment ; and thus, in discussing this theory, the physicist is compelled in great measure to adopt the qualitative methods of the biologist, rather than the quantitative ones which he would prefer.

The telescope seems to confirm the general correctness of Laplace's hypothesis. Thus, for example, the great nebula in Andromeda presents a grand illustration of what we may take to be a planetary system in course of formation. In it we see the central condensation surrounded by a more or less ring-like nebulosity, and in one of the rings there appears to be a subordinate condensation.

Nevertheless it is hardly too much to say that every stage in the supposed process presents to us some difficulty or impossibility. Thus we

ask whether a mass of gas of almost inconceivable tenuity can really rotate all in one piece, and whether it is not more probable that there would be a central whirlpool surrounded by more slowly-moving parts. Again, is there any sufficient reason to suppose that a series of intermittent efforts would lead to the detachment of distinct rings, and is not a continuous outflow of gas from the equator more probable?

The ring of Saturn seems to have suggested the theory to Laplace; but to take it as a model leads us straight to a quite fundamental difficulty. If a ring of matter ever concentrates under the influence of its mutual attraction, it can only do so round the centre of gravity of the whole ring. Therefore the matter forming an approximately uniform ring, if it concentrates at all, can only fall in on the parent planet and be re-absorbed. Some external force other than the mutual attraction of the matter forming the ring, and therefore not provided by the theory, seems necessary to effect the supposed concentration. The only way of avoiding this difficulty is to suppose the ring to be ill-balanced or lop-sided; in this case, provided the want of balance is pronounced enough, concentration will take place round a point inside the ring but outside the planet. Many writers assume that the present distances of the planets preserve the dimensions of the primitive rings; but the argument that a ring can only aggregate about its centre of gravity, which I do not recollect to have seen before, shows that such cannot be the case.

The concentration of an ill-balanced or broken ring on an interior point would necessarily generate a planet with direct rotation—that is to say, rotating in the same direction as the earth. But several writers, and notably Faye, endeavour to show—erroneously as I think—that a retrograde rotation should be normal, and they are therefore driven to make various complicated suppositions to explain the observed facts. But I do not claim to have removed the difficulty, only to have shifted it; for the satellites of Neptune, and presumably the planet itself, have retrograde rotations; and, lastly, the astonishing discovery has just been made by William Pickering of a ninth retrograde satellite of Saturn, while the rotations of the eight other satellites, of the ring and of the planet itself, are direct. Finally, I express a doubt as to whether the telescope does really exactly confirm the hypothesis of Laplace, for I imagine that what we see indicates a spiral rather than a ring-like division of nebulae.¹

This is not the time to pursue these considerations further, but enough has been said to show that the Nebular Hypothesis cannot be considered as a connected intelligible whole, however much of truth it may contain.

In the first theory which I sketched as to the origin of the sun and planets, we supposed them to grow by the accretions of meteoric wanderers in space, and this hypothesis is apparently in fundamental disagreement with the conception of Laplace, who watches the transformations

¹ Professor Chamberlin, of Chicago, has recently proposed a modified form of the Nebular Hypothesis, in which he contends that the spiral form is normal. See *Year Book* No. 3 for 1904 of the Carnegie Institution of Washington, pp. 195-258.

of a continuous gaseous nebula. Some years ago a method occurred to me by which these two discordant schemes of origin might perhaps be reconciled. A gas is not really continuous, but it consists of a vast number of molecules moving in all directions with great speed and frequently coming into collision with one another. Now I have ventured to suggest that a swarm of meteorites would, by frequent collisions, form a medium endowed with so much of the mechanical properties of a gas as would satisfy Laplace's conditions. If this is so, a nebula may be regarded as a quasi-gas, whose molecules are meteorites. The gaseous luminosity which undoubtedly is sent out by nebulae would then be due only to incandescent gas generated by the clash of meteorites, while the dark bodies themselves would remain invisible. Sir Norman Lockyer finds spectroscopic evidence which led him long ago to some such view as this, and it is certainly of interest to find in his views a possible means of reconciling two apparently totally discordant theories.¹ However, I do not desire to lay much stress on my suggestion, for without doubt a swarm of meteors could only maintain the mechanical properties of a gas for a limited time, and, as pointed out by Professor Chamberlin, it is difficult to understand how a swarm of meteorites moving indiscriminately in every direction could ever have come into existence. But my paper may have served to some extent to suggest to Chamberlin his recent modification of the Nebular Hypothesis, in which he seeks to reconcile Laplace's view with a meteoritic origin of the planetary system.²

We have seen that, in order to explain the genesis of planets according to Laplace's theory, the rings must be ill-balanced or even broken. If the ring were so far from being complete as only to cover a small segment of the whole circumference, the true features of the occurrences in the births of planets and satellites might be better represented by conceiving the detached portion of matter to have been more or less globular from the first, rather than ring-shaped. Now this idea introduces us to a group of researches whereby mathematicians have sought to explain the birth of planets and satellites in a way which might appear, at first sight, to be fundamentally different from that of Laplace.

The solution of the problem of evolution involves the search for those persistent or stable forms which biologists would call species. The species of which I am now going to speak may be grouped in a family, which comprises all those various forms which a mass of rotating liquid is capable of assuming under the conjoint influences of gravitation and rotation. If the earth were formed throughout of a liquid of the same density, it would be one of the species of this family; and indeed these researches date back to the time of Newton, who was the first to explain the figures of planets.

The ideal liquid planets we are to consider must be regarded as work-

¹ Newcomb considers the objections to Lockyer's theory insuperable. See p. 190 of *The Stars*, John Murray, London, 1904.

² See preceding reference to Chamberlin's Paper.

ing models of actuality, and inasmuch as the liquid is supposed to be incompressible, the conditions depart somewhat widely from those of reality. Hence, when the problem has been solved, much uncertainty remains as to the extent to which our conclusions will be applicable to actual celestial bodies.

We begin, then, with a rotating liquid planet like the earth, which is the first stable species of our family. We next impart in imagination more rotation to this planet, and find by mathematical calculation that its power of resistance to any sort of disturbance is less than it was. In other words, its stability declines with increased rotation, and at length we reach a stage at which the stability just vanishes. At this point the shape is a transitional one, for it is the beginning of a new species with different characteristics from the first, and with a very feeble degree of stability or power of persistence. As a still further amount of rotation is imparted, the stability of the new species increases to a maximum and then declines until a new transitional shape is reached and a new species comes into existence. In this way we pass from species to species with an ever-increasing amount of rotation.

The first or planetary species has a circular equator like the earth ; the second species has an oval equator, so that it is something like an egg spinning on its side on a table ; in the third species we find that one of the two ends of the egg begins to swell, and that the swelling gradually becomes a well-marked protrusion or filament. Finally the filamentous protrusion becomes bulbous at its end, and is only joined to the main mass of liquid by a gradually thinning neck. The neck at length breaks, and we are left with two separated masses which may be called planet and satellite. It is fair to state that the actual rupture into two bodies is to some extent speculative, since mathematicians have hitherto failed to follow the whole process to the end.

In this ideal problem the successive transmutations of species are brought about by gradual additions to the amount of rotation with which the mass of liquid is endowed. It might seem as if this continuous addition to the amount of rotation were purely arbitrary and could have no counterpart in nature. But real bodies cool and contract in cooling, and, since the scale of magnitude on which our planet is built is immaterial, contraction will produce exactly the same effect on shape as augmented rotation. I must ask you, then, to believe that the effects of an apparently arbitrary increase of rotation may be produced by cooling.

The figures which I succeeded in drawing, by means of rigorous calculation, of the later stages of this course of evolution, are so curious as to remind one of some such phenomenon as the protrusion of a filament of protoplasm from a mass of living matter, and I suggest that we may see in this almost life-like process the counterpart of at least one form of the birth of double stars, planets, and satellites.

As I have already said, Newton determined the first of these figures ; Jacobi found the second, and Poincaré indicated the existence of the

third, in a paper which is universally regarded as one of the masterpieces of applied mathematics ; finally I myself succeeded in determining the exact form of Poincaré's figure, and in proving that it is a true stable shape.

My Cambridge colleague Jeans has also made an interesting contribution to the subject by discussing a closely analogous problem, and he has besides attacked the far more difficult case where the rotating fluid is a compressible gas. In this case also he finds a family of types, but the conception of compressibility introduced a new set of considerations in the transitions from species to species. The problem is, however, of such difficulty that he had to rest content with results which were rather qualitative than strictly quantitative.

This group of investigations brings before us the process of the birth of satellites in a more convincing form than was possible by means of the general considerations adduced by Laplace. It cannot be doubted that the supposed Laplacian sequence of events possesses a considerable element of truth, yet these latter schemes of transformation can be followed in closer detail. It seems, then, probable that both processes furnish us with crude models of reality, and that in some cases the first and in others the second is the better representative.

The moon's mass is one-eightieth of that of the earth, whereas the mass of Titan, the largest satellite in the solar system, is $\frac{1}{4800}$ of that of Saturn. On the ground of this great difference between the relative magnitudes of all other satellites and of the moon, it is not unreasonable to suppose that the mode of separation of the moon from the earth may also have been widely different. The theory of which I shall have next to speak claims to trace the gradual departure of the moon from an original position not far removed from the present surface of the earth. If this view is correct, we may suppose that the detachment of the moon from the earth occurred as a single portion of matter, and not as a concentration of a Laplacian ring.

If a planet is covered with oceans of water and air, or if it is formed of plastic molten rock, tidal oscillations must be generated in its mobile parts by the attractions of its satellites and of the sun. Such movements must be subject to frictional resistance, and the planet's rotation will be slowly retarded by tidal friction in much the same way that a fly-wheel is gradually stopped by any external cause of friction. Since action and reaction are equal and opposite, the action of the satellites on the planet, which causes the tidal friction of which I speak, must correspond to a reaction of the planet on the motion of the satellites.

At any moment of time we may regard the system composed of the rotating planet with its attendant satellite as a stable species of motion, but the friction of the tides introduces forces which produce a continuous, although slow, transformation in the configuration. It is, then, clearly of interest to trace backwards in time the changes produced by such a continuously acting cause, and to determine the initial condition from which

the system of planet and satellite must have been slowly degrading. We may also look forward, and discover whither the transformation tends.

Let us consider, then, the motion of the earth and moon revolving in company round the sun, on the supposition that the friction of the tides in the earth is the only effective cause of change. We are, in fact, to discuss a working model of the system, analogous to those of which I have so often spoken before.

This is not the time to attempt a complete exposition of the manner in which tidal friction gives rise to the action and reaction between planet and satellite, nor shall I discuss in detail the effects of various kinds which are produced by this cause. It must suffice to set forth the results in their main outlines, and, as in connection with the topic of evolution retrospect is perhaps of greater interest than prophecy, I shall begin with the consideration of the past.

At the present time the moon, moving at a distance of 240,000 miles from the earth, completes her circuit in twenty-seven days. Since a day is the time of one rotation of the earth on its axis, the angular motion of the earth is twenty-seven times as rapid as that of the moon.

Tidal friction acts as a brake on the earth, and therefore we look back in retrospect to times when the day was successively twenty-three, twenty-two, twenty-one of our present hours in length, and so on backward to still shorter days. But during all this time the reaction on the moon was at work, and it appears that its effect must have been such that the moon also revolved round the earth in a shorter period than it does now ; thus the month also was shorter in absolute time than it now is. These conclusions are absolutely certain, although the effects on the motions of the earth and of the moon are so gradual that they can only doubtfully be detected by the most refined astronomical measurements.

We take the 'day,' regarding it as a period of variable length, to mean the time occupied by a single rotation of the earth on its axis ; and the 'month,' likewise variable in absolute length, to mean the time occupied by the moon in a single revolution round the earth. Then, although there are now twenty-seven days in a month, and although both day and month were shorter in the past, yet there is, so far, nothing to tell us whether there were more or less days in the month in the past. For if the day is now being prolonged more rapidly than the month, the number of days in the month was greater in the past than it now is ; and if the converse were true, the number of days in the month was less.

Now it appears from mathematical calculation that the day must now be suffering a greater degree of prolongation than the month, and accordingly in retrospect we look back to a time when there were more days in the month than at present. That number was once twenty-nine, in place of the present twenty-seven ; but the epoch of twenty-nine days in the month is a sort of crisis in the history of moon and earth, for yet earlier the day was shortening less rapidly than the month. Hence, earlier than

the time when there were twenty-nine days in the month, there was a time when there was a reversion to the present smaller number of days.

We thus arrive at the curious conclusion that there is a certain number of days to the month, namely twenty-nine, which can never have been exceeded, and we find that this crisis was passed through by the earth and moon recently ; but, of course, a recent event in such a long history may be one which happened some millions of years ago.

Continuing our retrospect beyond this crisis, both day and month are found continuously shortening, and the number of days in the month continues to fall. No change in conditions which we need pause to consider now supervenes, and we may ask at once, what is the initial stage to which the gradual transformation points ? I say, then, that on following the argument to its end the system may be traced back to a time when the day and month were identical in length, and were both only about four or five of our present hours. The identity of day and month means that the moon was always opposite to the same side of the earth ; thus at the beginning the earth always presented the same face to the moon, just as the moon now always shows the same face to us. Moreover, when the month was only some four or five of our present hours in length the moon must have been only a few thousand miles from the earth's surface—a great contrast with the present distance of 240,000 miles.

It might well be argued from this conclusion alone that the moon separated from the earth more or less as a single portion of matter at a time immediately antecedent to the initial stage to which she has been traced. But there exists a yet more weighty argument favourable to this view, for it appears that the initial stage is one in which the stability of the species of motion is tottering, so that the system presents the characteristic of a transitional form, which we have seen to denote a change of type or species in a previous case.

In discussing the transformations of a liquid planet we saw the tendency of the single mass to divide into two portions, although we failed to extend the rigorous argument back to the actual moment of separation ; and now we seem to reach a similar crisis from the opposite end, when in retrospect we trace back the system to two masses of unequal size in close proximity with one another. The argument almost carries conviction with it, but I have necessarily been compelled to pass over various doubtful points.

Time is wanting to consider other subjects worthy of notice which arise out of this problem, yet I wish to point out that the earth's axis must once have been less tilted over with reference to the sun than it is now, so that the obliquity of the ecliptic receives at least a partial explanation. Again, the inclination of the moon's orbit may be in great measure explained ; and, lastly, the moon must once have moved in a nearly circular path. The fact that tidal friction is competent to explain

the eccentricity of an orbit has been applied in a manner to which I shall have occasion to return hereafter.

In my paper on this subject I summed up the discussion in the following words, which I still see no reason to retract :—

‘The argument reposes on the imperfect rigidity of solids, and on the internal friction of semi-solids and fluids ; these are *veræ causee*. Thus changes of the kind here discussed must be going on, and must have gone on in the past. And for this history of the earth and moon to be true throughout it is only necessary to postulate a sufficient lapse of time, and that there is not enough matter diffused through space materially to resist the motions of the moon and earth in perhaps several hundred million years.

‘It hardly seems too much to say that granting these two postulates and the existence of a primeval planet, such as that above described, then a system would necessarily be developed which would bear a strong resemblance to our own.

‘A theory, reposing on *veræ causee*, which brings into quantitative correlation the lengths of the present day and month, the obliquity of the ecliptic, and the inclination and eccentricity of the lunar orbit, must, I think, have strong claims to acceptance.’¹

We have pursued the changes into the past, and I will refer but shortly to the future. The day and month are both now lengthening, but the day changes more quickly than the month. Thus the two periods tend again to become equal to one another, and it appears that when that goal is reached both day and month will be as long as fifty-five of our present days. The earth will then always show the same face to the moon, just as it did in the remotest past. But there is a great contrast between the ultimate and initial conditions, for the ultimate stage, with day and month both equal to fifty-five of our present days, is one of great stability in contradistinction to the vanishing stability which we found in the initial stage.

Since the relationship between the moon and earth is a mutual one, the earth may be regarded as a satellite of the moon, and if the moon rotated rapidly on her axis, as was probably once the case, the earth must at that time have produced tides in the moon. The mass of the moon is relatively small, and the tides produced by the earth would be large ; accordingly the moon would pass through the several stages of her history much more rapidly than the earth. Hence it is that the moon has already advanced to that condition which we foresee as the future fate of the earth, and now always shows to us the same face.

If the earth and moon were the only bodies in existence, this ultimate stage when the day and month were again identical in length, would be one of absolute stability, and therefore eternal ; but the presence of the sun introduces a cause for yet further changes. I do not, however, propose to pursue the history to this yet remoter futurity, because our system

must contain other seeds of decay which will probably bear fruit before these further transformations could take effect.

If, as has been argued, tidal friction has played so important a part in the history of the earth and moon, it might be expected that the like should be true of the other planets and satellites, and of the planets themselves in their relationship to the sun. But numerical examination of the several cases proves conclusively that this cannot have been the case. The relationship of the moon to the earth is in fact quite exceptional in the solar system, and we have still to rely on such theories as that of Laplace for the explanation of the main outlines of the solar system.

I have as yet only barely mentioned the time occupied by the sequence of events sketched out in the various schemes of cosmogony, and the question of cosmical time is a thorny and controversial one.

Our ideas are absolutely blank as to the time requisite for the evolution according to Laplace's nebular hypothesis. And again, if we adopt the meteoritic theory, no estimate can be formed of the time required even for an ideal sun, with its attendant planet Jove, to sweep up the wanderers in space. We do know, indeed, that there is a continuous gradation from stable to unstable orbits, so that some meteoric stones may make thousands or millions of revolutions before meeting their fate by collision. Accordingly, not only would a complete absorption of all the wanderers occupy an infinite time, but also the amount of the refuse of the solar system still remaining scattered in planetary space is unknown. And, indeed, it is certain that the process of clearance is still going on, for the earth is constantly meeting meteoric stones, which, penetrating the atmosphere, become luminous through the effects of the frictional resistance with which they meet.

All we can assert of such theories is that they demand enormous intervals of time as estimated in years.

The theory of tidal friction stands alone amongst these evolutionary speculations in that we can establish an exact but merely relative time-scale for every stage of the process. It is true that the value in years of the unit of time remains unknown, and it may be conjectured that the unit has varied to some extent as the physical condition of the earth has gradually changed.

It is, however, possible to determine a period in years which must be shorter than that in which the whole history is comprised. If at every moment since the birth of the moon tidal friction had always been at work in such a way as to produce the greatest possible effect, then we should find that sixty million years would be consumed in this portion of evolutionary history. The true period must be much greater, and it does not seem extravagant to suppose that 500 to 1,000 million years may have elapsed since the birth of the moon.

Such an estimate would not seem extravagant to geologists who have, in various ways, made exceedingly rough determinations of geological periods. One such determination is derived from measures of the thick-

ness of deposited strata, and the rate of the denudation of continents by rain and rivers. I will not attempt to make any precise statement on this head, but I imagine that the sort of unit with which the geologist deals is 100 million years, and that he would not consider any estimate involving from one to twenty of such units as unreasonable.

Mellard Reade has attempted to determine geological time by certain arguments as to the rate of denudation of limestone rocks, and arrives at the conclusion that geological history is comprised in something less than 600 million years.¹ The uncertainty of this estimate is wide, and I imagine that geologists in general would not lay much stress on it.

Joly has employed a somewhat similar, but probably less risky, method of determination.² When the earth was still hot, all the water of the globe must have existed in the form of steam, and when the surface cooled that steam must have condensed as fresh water. Rain then washed the continents and carried down detritus and soluble matter to the seas. Common salt is the most widely diffused of all such soluble matter, and its transit to the sea is an irreversible process, because the evaporation of the sea only carries back to the land fresh water in the form of rain. It seems certain, then, that the saltiness of the sea is due to the washing of the land throughout geological time.

Rough estimates may be formed of the amount of river water which reaches the sea in a year, and the measured saltiness of rivers furnishes a knowledge of the amount of salt which is thus carried to the sea. A closer estimate may be formed of the total amount of salt in the sea. On dividing the total amount of salt by the annual transport Joly arrives at the quotient of about 100 millions, and thence concludes that geological history has occupied 100 million years. I will not pause to consider the several doubts and difficulties which arise in the working out of this theory. The uncertainties involved must clearly be considerable, yet it seems the best of all the purely geological arguments whence we derive numerical estimates of geological time. On the whole I should say that pure geology points to some period intermediate between 50 and 1,000 millions of years, but the upper limit is more doubtful than the lower. Thus far we do not find anything which renders the tidal theory of evolution untenable.

But the physicists have formed estimates in other ways which, until recently, seemed to demand in the most imperative manner a far lower scale of time. According to all theories of cosmogony, the sun is a star which became heated in the process of its condensation from a condition of wide dispersion. When a meteoric stone falls into the sun the arrest of its previous motion gives rise to heat, just as the blow of a horse's shoe on a stone makes a spark. The fall of countless meteoric stones, or the

¹ *Chemical Denudation in relation to Geological Time*, Bogue, London, 1879; or *Roy. Soc. January 23, 1879.*

² 'An Estimate of the Geological Age of the Earth,' *Trans. Roy. Dublin Soc.*, vol. vii. series iii., 1902, pp. 23-66.

condensation of a rarefied gas, was supposed to be the sole cause of the sun's high temperature.

Since the mass of the sun is known, the total amount of the heat generated in it, in whatever mode it was formed, can be estimated with a considerable amount of precision. The heat received at the earth from the sun can also be measured with some accuracy, and hence it is a mere matter of calculation to determine how much heat the sun sends out in a year. The total heat which can have been generated in the sun divided by the annual output gives a quotient of about 20 millions. Hence it seemed to be imperatively necessary that the whole history of the solar system should be comprised within some 20 millions of years.

This argument, which is due to Helmholtz, appeared to be absolutely crushing, and for the last forty years the physicists have been accustomed to tell the geologists that they must moderate their claims. But for myself I have always believed that the geologists were more nearly correct than the physicists, notwithstanding the fact that appearances were so strongly against them.

And now, at length, relief has come to the strained relations between the two parties, for the recent marvellous discoveries in physics show that concentration of matter is not the only source from which the sun may draw its heat.

Radium is a substance which is perhaps millions of times more powerful than dynamite. Thus it is estimated that an ounce of radium would contain enough power to raise 10,000 tons a mile above the earth's surface. Another way of stating the same estimate is this: the energy needed to tow a ship of 12,000 tons a distance of six thousand sea miles at 15 knots is contained in 22 ounces of radium. The *Saxon* probably burns five or six thousand tons of coal on a voyage of approximately the same length. Again, M. and Mme. Curie have proved that radium actually gives out heat,¹ and it has been calculated that a small proportion of radium in the sun would suffice to explain its present radiation. Other lines of argument tend in the same direction.²

Now we know that the earth contains radio-active materials, and it is safe to assume that it forms in some degree a sample of the materials of the solar system. Hence it is almost certain that the sun is radio-active also; and besides it is not improbable that an element with so heavy an atom as radium would gravitate more abundantly to the central condensation than to the outlying planets. In this case the sun should contain a larger proportion of radio-active material than the earth.

This branch of science is as yet but in its infancy, but we already see how unsafe it is to dogmatise on the potentialities of matter.

¹ Lord Kelvin has estimated the age of the earth from the rate of increase of temperature underground. But the force of his argument seems to be entirely destroyed by this result.

² See W. E. Wilson, *Nature*, July 9, 1903; and G. H. Darwin, *Nature*, Sept. 24, 1903.

It appears, then, that the physical argument is not susceptible of a greater degree of certainty than that of the geologists, and the scale of geological time remains in great measure unknown.

I have now ended my discussion of the solar system, and must pass on to the wider fields of the stellar universe.

Only a few thousand stars are visible with the unaided eye, but photography has revealed an inconceivably vast multitude of stars and nebulae, and every improvement in that art seems to disclose yet more and more. About twenty years ago the number of photographic objects in the heavens was roughly estimated at about 170 millions, and some ten years later it had increased to about 400 millions. Although Newcomb, in his recent book on 'The Stars,' refrains even from conjecturing any definite number, yet I suppose that the enormous number of 400 million must now be far below the mark, and photography still grows better year by year. It seems useless to consider whether the number of stars has any limit, for infinite number, space, and time transcend our powers of comprehension. We must then make a virtue of necessity, and confine our attention to such more limited views as seem within our powers.

A celestial photograph looks at first like a dark sheet of paper splashed with whitewash, but further examination shows that there is some degree of method in the arrangement of the white spots. It may be observed that the stars in many places are arranged in lines and sweeping trains, and chains of stars, arranged in roughly parallel curves, seem to be drawn round some centre. A surface splashed at hazard might present apparent evidence of system in a few instances, but the frequency of the occurrence in the heavens renders the hypothesis of mere chance altogether incredible.

Thus there is order of some sort in the heavens, and, although no reason can be assigned for the observed arrangement in any particular case, yet it is possible to obtain general ideas as to the succession of events in stellar evolution.

Besides the stars there are numerous streaks, wisps, and agglomerations of nebulosity, whose light we know to emanate from gas. Spots of intenser light are observed in less brilliant regions; clusters of stars are sometimes imbedded in nebulosity, while in other cases each individual star of a cluster stands out clear by itself. These and other observations force on us the conviction that the wispy clouds represent the earliest stage of development, the more condensed nebulae a later stage, and the stars themselves the last stage. This view is in agreement with the nebular hypothesis of Laplace, and we may fairly conjecture that the chains and lines of stars represent pre-existing streaks of nebulosity.

As a star cools it must change, and the changes which it undergoes constitute its life-history, hence the history of a star presents an analogy with the life of an individual animal. Now, the object which I have had

in view has been to trace types or species in the physical world through their transformations into other types. Accordingly it falls somewhat outside the scope of this address to consider the constitution and history of an individual star, interesting although those questions are. I may, however, mention that the constitution of gaseous stars was first discussed from the theoretical side by Lane, and subsequently more completely by Ritter. On the observational side the spectroscope has proved to be a powerful instrument in analysing the constitutions of the stars, and in assigning to them their respective stages of development.

If we are correct in believing that stars are condensations of matter originally more widely spread, a certain space surrounding each star must have been cleared of nebulosity in the course of its formation. Much thought has been devoted to the determination of the distribution of the stars in space, and although the results are lacking in precision, yet it has been found possible to arrive at a rough determination of the average distance from star to star. It has been concluded, from investigations into which I cannot enter, that if we draw a sphere round the sun with a radius of twenty million millions of miles,¹ it will contain no other star; if the radius were twice as great the sphere might perhaps contain one other star; a sphere with a radius of sixty million millions of miles will contain about four stars. This serves to give some idea of the extraordinary sparseness of the average stellar population; but there are probably in the heavens urban and rural districts, as on earth, where the stars may be either more or less crowded. The stars are moving relatively to one another with speeds which are enormous, as estimated by terrestrial standards, but the distances which separate us from them are so immense that it needs refined observation to detect and measure the movements.

Change is obviously in progress everywhere, as well in each individual nebula and star as in the positions of these bodies relatively to one another. But we are unable even to form conjectures as to the tendency of the evolution which is going on. This being so, we cannot expect, by considering the distribution of stars and nebulae, to find many illustrations of the general laws of evolution which I have attempted to explain; accordingly I must confine myself to the few cases where we at least fancy ourselves able to form ideas as to the stages by which the present conditions have been reached.

Up to a few years ago there was no evidence that the law of gravitation extended to the stars, and even now there is nothing to prove the transmission of gravity from star to star. But in the neighbourhood of many stars the existence of gravity is now as clearly demonstrated as within the solar system itself. The telescope has disclosed the double character of a large number of stars, and the relative motions of the pairs of companions have been observed with the same assiduity as that of the

¹ This is the distance at which the earth's distance from the sun would appear to be 1".

planets. When the relative orbit of a pair of binary or double stars is examined, it is found that the motion conforms exactly to those laws of Kepler which prove that the planets circle round the sun under the action of solar gravitation. The success of the hypothesis of stellar gravitation has been so complete that astronomers have not hesitated to explain the anomalous motion of a seemingly single star by the existence of a dark companion ; and it is interesting to know that the more powerful telescopes of recent times have disclosed, in at least two cases, a faintly luminous companion in the position which had been assigned to it by theory.

By an extension of the same argument, certain variations in the spectra of a considerable number of stars have been pronounced to prove them each to be really double, although in general the pair may be so distant that they will probably always remain single to our sight. Lastly, the variability in the light of other apparently single stars has proved them to be really double. A pair of stars may partially or wholly cover one another as they revolve in their orbit, and the light of the seemingly single star will then be eclipsed, just as a lighthouse winks when the light is periodically hidden by a revolving shutter. Exact measurements of the character of the variability in the light have rendered it possible not only to determine the nature of the orbit described, but even to discover the figures and densities of the two components which are fused together by the enormous distance of our point of view. This is a branch of astronomy to which much careful observation and skilful analysis has been devoted ; and I am glad to mention that Alexander Roberts, one of the most eminent of the astronomers who have considered the nature of variable stars, is a resident in South Africa.

I must not, however, allow you to suppose that the theory of eclipses will serve to explain the variability of all stars, for there are undoubtedly others whose periodicity must be explained by something in their internal constitution.

The periods of double stars are extremely various, and naturally those of short period have been the first noted ; in times to come others with longer and longer periods will certainly be discovered. A leading characteristic of all these double stars is that the two companions do not differ enormously in mass from one another. In this respect these systems present a strongly marked contrast with that of the sun, attended as it is by relatively insignificant planets.

In the earlier part of my address I showed how theory indicates that a rotating fluid body will as it cools separate into two detached masses. Mathematicians have not yet been able to carry their analysis far enough to determine the relative magnitudes of the two parts, but as far as we can see the results point to the birth of a satellite whose mass is a considerable fraction of that of its parent. Accordingly See (who devotes his attention largely to the astronomy of double stars), Roberts, and others consider that what they have observed in the heavens is in agreement

with the indications of theory. It thus appears that there is reason to hold that double stars have been generated by the division of primitive and more diffused single stars.

But if this theory is correct we should expect the orbit of a double star to be approximately circular ; yet this is so far from being the case that the eccentricity of the orbits of many double stars exceeds by far any of the eccentricities in the solar system. Now See has pointed out that when two bodies of not very unequal masses revolve round one another in close proximity the conditions are such as to make tidal friction as efficient as possible in transforming the orbit. Hence we seem to see in tidal friction a cause which may have sufficed not only to separate the two component stars from one another, but also to render the orbit eccentric.

I have thought it best to deal very briefly with stellar astronomy, in spite of the importance of the subject, because the direction of the changes in progress is in general too vague to admit of the formation of profitable theories.

We have seen that it is possible to trace the solar system back to a primitive nebula with some degree of confidence, and that there is reason to believe that the stars in general have originated in the same manner. But such primitive nebulae stand in as much need of explanation as their stellar offspring. Thus, even if we grant the exact truth of these theories, the advance towards an explanation of the universe remains miserably slight. Man is but a microscopic being relatively to astronomical space, and he lives on a puny planet circling round a star of inferior rank. Does it not then seem as futile to imagine that he can discover the origin and tendency of the universe as to expect a housefly to instruct us as to the theory of the motions of the planets? And yet, so long as he shall last, he will pursue his search, and will no doubt discover many wonderful things which are still hidden. We may indeed be amazed at all that man has been able to find out, but the immeasurable magnitude of the undiscovered will throughout all time remain to humble his pride. Our children's children will still be gazing and marvelling at the starry heavens, but the riddle will never be read.

British Association for the Advancement of Science.

LEICESTER, 1907.

ADDRESS

BY

SIR DAVID GILL, K.C.B., LL.D., D.Sc.,
F.R.S., HON. F.R.S.E., &c.,
PRESIDENT.

TO-NIGHT, for the first time in its history, the British Association meets in the ancient city of Leicester; and it now becomes my privilege to convey to you, Mr. Mayor, and to the citizens generally, an expression of our thanks for your kind invitation and for the hospitable reception which you have accorded to us.

Here in Leicester and last year in York the Association has followed its usual custom of holding its annual meeting somewhere in the United Kingdom; but in 1905 the meeting was, as you know, held in South Africa. Now, having myself only recently come from the Cape, I wish to take this opportunity of saying that this southern visit of the Association has, in my opinion, been productive of much good: wider interest in science has been created amongst colonists, juster estimates of the country and its problems have been formed on the part of the visitors, and personal friendships and interchange of ideas between thinking men in South Africa and at home have arisen which cannot fail to have a beneficial influence on the social, political, and scientific relations between these colonies and the mother country. We may confidently look for like results from the proposed visit of the Association to Canada in 1909.

One is tempted to take advantage of the wide publicity given to words from this Chair to speak at large in the cause of science, to insist upon the necessity for its wider inclusion in the education of our youth and the devotion of a larger measure of the public funds in aid of scientific research; to point to the supreme value of science as a means for the culture of those faculties which in man promote that knowledge which is power; and to show how dependent is the progress of a nation upon its scientific attainment.

But in recent years these truths have been prominently brought

before the Association from this Chair; they have been exhaustively demonstrated by Sir William Huggins from the Chair of the Royal Society, and now a special guild¹ exists for their enforcement upon the mind of the nation.

These considerations appear to warrant me in following the healthy custom of so many previous Presidents—viz., of confining their remarks mainly to those departments of science with which the labours of their lives have been chiefly associated.

The Science of Measurement.

Lord Kelvin in 1871 made a statement from the Presidential Chair of the Association at Edinburgh as follows: 'Accurate and minute measurement seems to the non-scientific imagination a less lofty and dignified work than the looking for something new. But nearly all the grandest discoveries of science have been the reward of accurate measurement and patient, long-continued labour in the minute sifting of numerical results.'

Besides the instances quoted by Lord Kelvin in support of that statement, we have perhaps as remarkable and typical an exemplification as any in Lord Rayleigh's long-continued work on the density of nitrogen which led him to the discovery of argon. We shall see presently that, true as Lord Kelvin's words are in regard to most fields of science, they are specially applicable as a guide in astronomy.

One of Clerk Maxwell's lectures in the Natural Philosophy Class at Marischall College, Aberdeen, when I was a student under him there, in the year 1859, ran somewhat as follows:—

A standard, as it is at present understood in England, is not a real standard at all; it is a rod of metal with lines ruled upon it to mark the yard, and it is kept somewhere in the House of Commons. If the House of Commons catches fire there may be an end of your standard. A copy of a standard can never be a real standard, because all the work of human hands is liable to error. Besides, will your so-called standard remain of a constant length? It certainly will change by temperature, it probably will change by age (that is, by the rearrangement or settling down of its component molecules), and I am not sure if it does not change according to the azimuth in which it is used. At all events, you must see that it is a very impractical standard—impractical because, if, for example, any one of you went to Mars or Jupiter, and the people there asked you what was your standard of measure, you could not tell them, you could not reproduce it, and you would feel very foolish. Whereas, if you told any capable physicist in Mars or Jupiter that you used some natural invariable standard, such as the wave-length of the D line of sodium vapour, he would be able to reproduce your yard or your inch, provided that you could tell him how many of such wave-lengths there were in your yard or your inch, and your standard would be available anywhere in the universe where sodium is found.

That was the whimsical way in which Clerk Maxwell used to impress

great principles upon us. We all laughed before we understood ; then some of us understood and remembered.

Now the scientific world has practically adopted Maxwell's form of natural standard. It is true that it names that standard the metre ; but that standard is not one-millionth of the Earth's quadrant in length, as it was intended to be ; it is merely a certain piece of metal approximately of that length.

It is true that the length of that piece of metal has been reproduced with more precision, and is known with higher accuracy in terms of many secondary standards, than is the length of any other standard in the world ; but it is, after all, liable to destruction and to possible secular change of length. For these reasons it cannot be scientifically described otherwise than as a piece of metal whose length at 0° C. at the epoch A.D. 1906 is $=1,553,164$ times the wave-length of the red line of the spectrum of cadmium when the latter is observed in dry air at the temperature of 15° C. of the normal hydrogen-scale at a pressure of 760 mm. of mercury at 0° C.

This determination, recently made by methods based on the interference of light-waves and carried out by MM. Perot and Fabry at the International Bureau of Weights and Measures, constitutes a real advance in scientific metrology. The result appears to be reliable within one ten-millionth part of the metre.

The length of the metre, in terms of the wave-length of the red line in the spectrum of cadmium, had been determined in 1892 by Michelson's method, with a mean result in almost exact accordance with that just quoted for the comparisons of 1906 ; but this agreement (within one part in ten millions) is due in some degree to chance, as the uncertainty of the earlier determination was probably ten times greater than the difference between the two independent results of 1892 and 1906.

We owe to M. Guillaume, of the same International Bureau, the discovery of the remarkable properties of the alloys of nickel and steel, and from the point of view of exact measurement the specially valuable discovery of the properties of that alloy which we now call 'invar.' He has developed methods for treatment of wires made from this alloy which render more permanent the arrangement of their constituent molecules. Thus these wires, with their attached scales, may, for considerable periods of time and under circumstances of careful treatment, be regarded as nearly invariable standards. With proper precautions, we have found at the Cape of Good Hope that these wires can be used for the measurement of base lines of the highest geodetic precision with all the accuracy attainable by the older and most costly forms of apparatus ; whilst with the new apparatus a base of 20 kilometres can be measured in less time and for less cost than one of a single kilometre with the older forms of measurement.

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The Great African Arc of Meridian.

In connection with the progress of geodesy, time only permits me to say a few words about the Great African arc on the 30th meridian, which it is a dream of my life to see completed.

The gap in the arc between the Limpopo and the previously executed triangulation in Rhodesia, which I reported to the Association at the Johannesburg meeting in 1905, has now been filled up. My own efforts, at 6,000 miles distance, had failed to obtain the necessary funds, but at Sir George Darwin's instance contributions were obtained from this Association, from the Royal Society and others, to the extent of half the estimated cost; the remaining half was met by the British South Africa Company. But for Darwin's happy intervention, which enabled me to secure the services of Captain Gordon and his party before the Transvaal Survey Organisation was entirely broken up, this serious gap in the great work would probably have long remained; for it is one thing to add to an existing undertaking of the kind, it is quite another to create a new organisation for a limited piece of work.

Since then Colonel (now Sir William) Morris has brought to a conclusion the reductions of the geodetic survey of the Transvaal and Orange River Colony, and his report is now in my hands for publication.

Dr. Rubin, under my direction, at the cost of the British South Africa Company, has carried the arc of meridian northwards to S. latitude $9^{\circ} 42'$, so that we have now continuous triangulation from Cape L'Agulhas to within fifty miles of the southern end of Lake Tanganyika; that is to say, a continuous geodetic survey extending over twenty-five degrees of latitude.

It happens that, for the adjustment of the international boundary between the British Protectorate and the Congo Free State, a topographic survey is at the present moment being executed northward along the 30th meridian from the northern border of German East Africa. A proposal on the part of the Royal Society, the Royal Geographical Society, the British Association, and the Royal Astronomical Society has been made to strengthen this work by carrying a geodetic triangulation through it along the 30th meridian, and thus adding $2\frac{1}{2}^{\circ}$ to the African arc. These Societies together guarantee 1,000*l.* towards the cost of the work, and ask for a like sum from Government to complete the estimated cost. The topographic survey will serve as the necessary reconnaissance. The topographic work will be completed by the end of January next, and the four following months offer the best season of the year for geodetic operations in these regions.

There is a staff of skilled officers and men on the spot sufficient to complete the work within the period mentioned, and the Intercolonial Council of the Transvaal and Orange River Colony most generously offers to lend the necessary geodetic instruments. The work will have to be done sooner or later, but if another expedition has to be organised for the

purpose the work will then cost from twice to three times the present amount. One cannot therefore doubt that His Majesty's Government will take advantage of the present offer and opportunity to vote the small sum required. This done, we cannot doubt that the German Government will complete the chain along the eastern side of Lake Tanganyika, which lies entirely within their territory. Indeed, it is no secret that the Berlin Academy of Sciences has already prepared the necessary estimates with a view to recommending action on the part of its Government.

Captain Lyons, who is at the head of the survey of Egypt, assures me that preliminary operations towards carrying the arc southwards from Alexandria have been begun, and we have perfect confidence that in his energetic hands the work will be prosecuted with vigour. In any case the completion of the African arc will rest largely in his hands. That arc, if ever my dream is realised, will extend from Cape L'Agulhas to Cairo, thence round the eastern shore of the Mediterranean and the islands of Greece, and there meet the triangulation of Greece itself, the latter being already connected with Struve's great arc, which terminates at the North Cape in lat. 70° N. This will constitute an arc of 105° in length—the longest arc of meridian that is measurable on the earth's surface.

The Solar Parallax.

Much progress has been made in the exact measurement of the great fundamental unit of astronomy—the solar parallax.

Early in 1877 I ventured to predict¹ that we should not arrive at any certainty as to the true value of the solar parallax from observations of transits of Venus, but that the modern heliometer applied to the measurement of angular distances between stars and the star-like images of minor planets would yield results of far higher precision.

The results of the observations of the minor planets Iris, Victoria, and Sappho at their favourable oppositions in the years 1888 and 1889, which were made with the co-operation of the chief heliometer and meridian observatories, fully justified this prediction.² The Sun's distance is now almost certainly known within one-thousandth part of its amount. The same series of observations also yielded a very reliable determination of the mass of the Moon.

The more recently discovered planet Eros, which in 1900 approached the Earth within one-third of the mean distance of the Sun, afforded a most unexpected and welcome opportunity for redetermining the solar parallax—an opportunity which was largely taken advantage of by the principal observatories of the northern hemisphere. Unfortunately the high northern declination of the planet prevented its observation at the Cape and other southern observatories. So far as the results have been

¹ 'The Determination of the Solar Parallax,' *The Observatory*, vol. i. p. 280.

² *Annals of the Cape Observatory*, vol. vi., part 6, p. 29.

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reduced and published¹ they give an almost exact accordance with the value of the solar parallax derived from the heliometer observations of the minor planets, Iris, Victoria, and Sappho in 1888 and 1889.

But in 1931 Eros will approach the Earth within one-sixth part of the Sun's mean distance, and the fault will rest with astronomers of that day if they do not succeed in determining the solar parallax within one ten-thousandth part of its amount.

To some of us who struggled so hard to arrive at a tenth part of this accuracy under the less favourable geometrical conditions that were available before the discovery of Eros, how enviable seems the opportunity !

And yet, if we come to think of it rightly, the true opportunity and the chief responsibility is ours, for *now* and not twenty years hence is the time to begin our preparation ; *now* is the time to study the origin of those systematic errors which undoubtedly attach to some of our photographic processes ; and then we ought to construct telescopes specially designed for the work. These telescopes should be applied to the charting of the stars near the path which Eros will describe at its opposition in 1931, and the resulting star-co-ordinates derived from the plates photographed by the different telescopes should be rigorously inter-compared. Then, if all the telescopes give identical results for the star-places, we can be certain that they will record without systematic error the position of Eros. If they do not give identical results, the source of the errors must be traced.

The planet will describe such a long path in the sky during the opposition of 1931 that it is already time to begin the meridian observations which are necessary to determine the places of the stars that are to be used for determining the constants of the plates. It is desirable, therefore, that some agreement should be come to with respect to selection of these reference-stars, in order that all the principal meridian observatories in the world may take part in observing them.

I venture to suggest that a Congress of Astronomers should assemble in 1908 to consider what steps should be taken with reference to the important opposition of Eros in 1931.

The Stellar Universe.

And now to pass from consideration of the dimensions of our solar system to the study of the stars, or other suns, that surround us.

To the lay mind it is difficult to convey a due appreciation of the value and importance of star-catalogues of precision. As a rule such catalogues have nothing whatever to do with discovery in the ordinary sense of the word, for the existence of the stars which they contain is generally well known beforehand ; and yet such catalogues are, in reality, by far the most valuable assets of astronomical research.

¹ *Monthly Notices R.A.S.*, Hinks, vol. lxiv. p. 725 ; Christie, vol. lxvii. p. 382.

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If it be desired to demarcate a boundary on the Earth's surface by astronomical methods, or to fix the position of any object in the heavens, it is to the accurate star catalogue that we must refer for the necessary data. In that case the stars may be said to resemble the trigonometrical points of a survey, and we are only concerned to know from accurate catalogues their positions in the heavens at the epoch of observation. But in another and grander sense the stars are not mere landmarks, for each has its own apparent motion in the heavens which may be due in part to the absolute motion of the star itself in space, or in part to the motion of the solar system by which our point of view of surrounding stars is changed.

If we desire to determine these motions and to ascertain something of the general conditions which produce them, if we would learn something of the dynamical conditions of the universe and something of the velocity and direction of our own solar system through space, it is to the accurate star catalogues of widely separated epochs that we must turn for a chief part of the requisite data.

The value of a star-catalogue of precision for present purposes of cosmic research varies as the square of its age and the square of its accuracy. We cannot alter the epoch of our observations, but we can increase their value fourfold by doubling their accuracy. Hence it is that many of our greater astronomers have devoted their lives chiefly to the accumulation of meridian observations of high precision, holding the view that to advance such precision is the most valuable service to science they could undertake, and comforted in their unselfish and laborious work only by the consciousness that they are preparing a solid foundation on which future astronomers may safely raise the superstructure of sound knowledge.

But since the extension of our knowledge of the system of the universe depends quite as much on past as on future research, it may be well, before determining upon a programme for the future, to consider briefly the record of meridian observation in the past for both hemispheres.

The Comparative State of Astronomy in the Northern and Southern Hemispheres.

It seems probable that the first express reference to southern constellations in known literature occurs in the Book of Job (ix. 9): 'Which maketh Arcturus, Orion, and Pleiades, and the chambers of the south.' Schiaparelli's strongly supported conjecture is that the expression 'chambers of the south,' taken with its context, signifies the brilliant stellar region from Canopus to α Centauri, which includes the Southern Cross and coincides with the most brilliant portion of the Milky Way.

About the year 750 B.C. (the probable date of the Book of Job) all these stars culminated at altitudes between 5° and 16° when viewed from

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the latitude of Judæa ; but now, owing to precessional change, they can only be seen in a like striking manner from a latitude about 12° further south.

The words of Dante have unquestionably originated the wonderful net of poetic fancy that has been woven about the asterism, which we now call Crux.

To the right hand I turned, and fixed my mind
On the other pole attentive, where I saw
Four stars ne'er seen before save by the ken
Of our first parents—Heaven of their rays
Seemed joyous. O thou northern site ! bereft
Indeed, and widowed, since of these deprived.

All the commentators agree that Dante here referred to the stars of the Southern Cross.

Had Dante any imperfect knowledge of the existence of these stars, any tradition of their visibility from European latitudes in remote centuries, so that he might poetically term them the stars of our first parents ?

Ptolemy catalogues them as 31, 32, 33, and 34 Centauri, and they are clearly marked on the Borgian globe described by Assemanus in 1790. This globe was constructed by an Arabian in Egypt : it bears the date 622 Hegira, corresponding with A.D. 1225, and it is possible that Dante may have seen it.

Amerigo Vespucci, as he sailed in tropical seas, apparently recognised in what we now call Crux the four luminous stars of Dante ; for in 1501 he claimed to be the first European to have looked upon the stars of our first parents. His fellow-voyager, Andrea Corsali, wrote about the same time to Giuliano di Medici describing 'the marvellous cross, the most glorious of all the celestial signs.'

Thus much mysticism and romance have been woven about this constellation, with the result that exaggerated notions of its brilliancy have been formed, and to most persons its first appearance, when viewed in southern latitudes, is disappointing.

To those, however, who view it at upper culmination for the first time from a latitude a little south of the Canary Islands, and who at the same time make unconsciously a mental allowance for the absorption of light to which one is accustomed in the less clear skies of Northern Europe, the sight of the upright cross, standing as if fixed to the horizon, is a most impressive one. I at least found it so on my first voyage to the Cape of Good Hope. But how much more strongly must it have appealed to the mystic and superstitious minds of the early navigators as they entered the unexplored seas of the northern tropic ! To them it must have appeared the revered image of the Cross pointing the way on their southward course—a symbol and sign of Hope and Faith on their entry to the unknown.

The first general knowledge of the brighter stars of the southern

hemisphere we owe to Frederick de Hautman, who commanded a fleet sent by the Dutch Government in 1595 to the Far East for the purpose of exploring Japan. Hautman was wrecked and taken prisoner at Sumatra, and whilst there he studied the language of the natives and made observations of the positions and magnitudes of the fixed stars of the southern hemisphere.¹

Our distinguished countryman Halley visited St. Helena in 1677 for the purpose of cataloguing the stars of the southern hemisphere. He selected a station now marked Halley's Mount on the Admiralty chart of the island. I have visited the site, and the foundations of the observatory still remain. Halley's observations were much hindered by cloud. On his return to England, Halley in 1679 published his '*Catalogus Stellarum Australium*,' containing the magnitudes, latitudes, and longitudes of 341 stars, which, with the exception of seven, all belonged to the southern hemisphere.

But the first permanently valuable astronomical work in the southern hemisphere was done in 1751-52 by the Abbé de Lacaille. He selected the Cape of Good Hope as the scene of his labours, because it was then perhaps the only spot in the world situated in a considerable southern latitude which an unprotected astronomer could visit in safety, and where the necessary aid of trained artisans to erect his observatory could be obtained. Lacaille received a cordial welcome at the hands of the Dutch governor Tulbagh: he erected his observatory in Cape Town, made a catalogue of nearly 10,000 stars, observed the opposition of Mars, and measured a short arc of meridian all in the course of a single year. Through his labours the Cape of Good Hope became the birthplace of astronomy and geodesy in the southern hemisphere.

Bradley was laying the foundations of exact astronomy in the northern hemisphere at the time when Lacaille laboured at the Cape. But Bradley had superior instruments to those of Lacaille and much longer time at his disposal. Bradley's work is now the basis on which the fair superstructure of modern astronomy of precision rests. His labours were continued by his successors at Greenwich and by a long series of illustrious men like Piazzzi, Groombridge, Bessel, Struve, and Argelander. But in the southern hemisphere the history of astronomy is a blank for seventy years from the days of Lacaille.

We owe to the establishment of the Royal Observatory at the Cape by an Order in Council of 1820 the first successful step towards the foundation of astronomy of high precision in the southern hemisphere.

Time does not permit me to trace in detail the labours of astronomers in the southern hemisphere down to the present day; and this is the less necessary because in a recent Presidential Address to the South African Philosophical Society² I have given in great part that history in

¹ The resulting catalogue of 304 stars is printed as an appendix to Hautman's *Vocabulary of the Malay Language*, published at Amsterdam in 1603.

² *Trans. South African Phil. Soc.*, vol. xiv. part 2.

considerable detail. But I have not there made adequate reference to the labours of Dr. Gould and Dr. Thome at Cordoba. To their labours, combined with the work done under Stone at the Cape, we owe the fact that for the epoch 1875 the meridian sidereal astronomy of the southern hemisphere is nearly as well provided for as that of the northern. The point I wish to make is that the facts of exact sidereal astronomy in the southern hemisphere may be regarded as dating nearly a hundred years behind those of the northern hemisphere.

The Constitution of the Universe.

It was not until 1718, when Edmund Halley, afterwards Astronomer Royal of England, read a paper before the Royal Society,¹ entitled 'Considerations on the Change of the Latitudes of Some of the Principal Fixt Stars,' that any definite facts were known about the constitution of the universe. In that paper Halley, who had been investigating the precession of the equinoxes, says: 'But while I was upon this enquiry I was surprized to find the Latitudes of three of the principal Stars in heaven directly to contradict the supposed greater obliquity of the Ecliptick, which seems confirmed by the Latitudes of most of the rest.'

This is the first mention in history of an observed change in the relative position of the so-called fixed stars—the first recognition of what we now call 'proper motion.'

Tobias Mayer, in 1760, seems to have been the first to recognise that if our Sun, like other stars, has motion in space, that motion must produce apparent motion amongst the surrounding stars; for in a paper to the Gottingen Academy of Sciences he writes: 'If the Sun, and with it the planets and the Earth which we inhabit, tended to move directly towards some point in the heavens, all the stars scattered in that region would seem to gradually move apart from each other, whilst those in the opposite quarter would mutually approach each other. In the same manner one who walks in the forest sees the trees which are before him separate, and those that he leaves behind approach each other.' No statement of the matter could be more clear; but Mayer, with the meagre data at his disposal, came to the conclusion that 'the motions of the stars are not governed by the above or any other common law, but belong to the stars themselves.'

Sir William Herschel, in 1783, made the first attempt to apply, with any measure of success, Mayer's principle to a determination of the direction and amount of the solar motion in space.² He derived, as well as he could from existing data, the proper motions of fourteen stars, and arrived by estimation at the conclusion that the Sun's motion in space is nearly in the direction of the star λ Herculis, and that 80 per cent. of the apparent motions of the fourteen stars in question could be assigned to this common origin.

¹ *Phil. Trans.*, 1718, p. 738.

² *Ibid.*, 1783, p. 247.

This conclusion rests in reality upon a very slight basis, but the researches of subsequent astronomers show that it was an amazing accidental approach to truth—indeed, a closer approximation than Herschel's subsequent determinations of 1805 and 1806, which rested on wider and better data.¹

Consider for a moment the conditions of the problem. If all the stars except our Sun were at rest in space, then, in accordance with Mayer's statement, just quoted, all the stars would have apparent motions on great circles of the sphere away from the apex and towards the ant-apex of the solar motion. That is to say, if the position of each star of which the apparent motion is known was plotted on the surface of a sphere and a line with an arrow-head drawn through each star showing the direction of its motion on the sphere, then it should be possible to find a point on the sphere such that a great circle drawn from this point through any star would coincide with the line of direction of that star's proper motion. The arrow-heads would all point to that intersection of the great circles which is the antapex of the solar motion, and the other point of intersection of the great circles would be the apex, that is to say, the direction of the Sun's motion in space.

But as the apparent stellar motions are small and only determinable with a considerable percentage of error, it would be impossible to find any point on the sphere such that every great circle passing through it and any particular star, would in every case be coincident with the observed direction of motion of that star.

Such discordances would, on our original assumption, be due to errors of observation, but in reality much larger discordances will occur, which are due to the fact that the other stars (or suns) have independent motions of their own in space. This at once creates a new difficulty, viz., that of defining an absolute locus in space. The human mind may exhaust itself in the effort, but it can never solve the problem. We can imagine, for example, the position of the Sun at any moment to be defined with reference to any number of surrounding stars, but by no effort of imagination can we devise means of defining the *absolute* position of a body in space without reference to surrounding material objects. If, therefore, the referring objects have unknown motions of their own, the rigour of the definition is lost.

What we call the observed proper motion of a star has three possible sources of origin :—

1. The *parallactic motion*, or the effect of our Sun's motion through space, whereby our point of view of surrounding celestial objects is changed.
2. The *peculiar* or particular motion of the star, *i.e.*, its own *absolute* motion in space.
3. That part of the observed or tabular motion which is due to inevitable error of observation.

¹ *Phil. Trans*, 1805, p. 233 ; 1806, p. 205.

In all discussions of the solar motion in space, from that of Herschel down till a recent date, it has been assumed that the peculiar motions of the stars are arranged at random, and may therefore be considered zero in the mean of a considerable number of them. It is then possible to find such a value for the Precession, and such a common apex for the solar motion as shall leave the residual peculiar motions of the stars under discussion to be in the mean = zero. That is to say, we refer the motion of the Sun in space to the centre of gravity of all the stars considered in the discussion, and regard that centre of gravity as immovable in space.

In order to proceed rigorously, and especially to determine the amount as well as the direction of the Sun's motion in space, we ought to know the parallax of every star employed in the discussion, as well as its proper motion. In the absence of such data it has been usual to start from some such assumption as the following: the stars of a particular magnitude are roughly at the same distance; those of different classes of magnitude may be derived from the hypothesis that on the average they have all equal absolute luminosity.

The assumption is not a legitimate one —

1. Because of the extreme difference in the absolute luminosity of stars.
2. Because it implies that the average absolute luminosity of stars is the same in all regions of space.

The investigation has been carried out by many successive astronomers on these lines with fairly accordant results as to the position of the solar apex, but with very unsatisfactory results as to the distances of the fixed stars.¹ In order to judge how far the magnitude (or brightness) of a star is an index of its probable distance, we must have evidence from direct determinations of stellar parallax.

¹ Argelander, *Mém. présentés à l'Acad. Imp. des Sciences St. Pétersbourg*, tome iii

Lundahl, *Astron. Nachrichten*, **398**, 209.

Argelander, *Astron. Nachrichten*, **398**, 210.

Otto Struve, *Mém. Acad. des Sciences St. Pétersbourg*, vi^e série, Math. et Phys., tome iii. p. 17.

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Ludwig Struve, *Mém. Acad. St. Pétersbourg*, vii^e série, tome xxxv. No. 3.

Stellar Parallax.

To extend exact measurement from our own solar system to that of other suns and other systems may be regarded as the supreme achievement of practical astronomy. So great are the difficulties of the problem, so minute the angles involved, that it is but in comparatively recent years that any approximate estimate could be formed of the true parallax of any fixed star. Bradley felt sure that if the star γ Draconis had a parallax of 1" he would have detected it. Henderson by 'the minute sifting of the numerical results' of his own meridian observations of α Centauri, made at the Cape of Good Hope in 1832-33, first obtained certain evidence of the measurable parallax of any fixed star. He was favoured in this discovery by the fact that the object he selected happened to be, so far as we yet know, the nearest sun to our own. Shortly afterwards Struve obtained evidence of a measurable parallax for α Lyrae and Bessel for 61 Cygni. Astronomers hailed with delight this bursting of the constraints which our imperfect means imposed on research. But for the great purposes of cosmical astronomy what we are chiefly concerned to know is not what is the parallax of this or that particular star, but rather what is the average parallax of a star having a particular magnitude and proper motion. The prospect of even an ultimate approximate attainment of this knowledge seemed remote. The star α Lyrae is one of the brightest in the heavens; the star 61 Cygni one that had the largest proper motion known at the time; whilst α_2 Centauri is not only a very bright star, but it has also a large proper motion. The parallaxes of these stars must therefore in all probability be large compared with the parallax of the average star; but yet to determine them with approximate accuracy long series of observations by the greatest astronomers and with the finest instruments of the day seemed necessary.

Subsequently various astronomers investigated the parallaxes of other stars having large proper motions, but it was only in 1881, at the Cape of Good Hope, that general research on stellar parallax was instituted.¹ Subsequently at Yale and at the Cape of Good Hope the work was continued on cosmical lines with larger and improved heliometers.² By the introduction of the reversing prism and by other practical refinements the possibilities of systematic error were eliminated, and the accidental errors of observation reduced within very small limits.

These researches brought to light the immense diversity in the absolute luminosity and velocity of motion of different stars. Take the following by way of example :—

Our nearest neighbour amongst the stars, α_2 Centauri, has a parallax

¹ *Mem. R.A.S.*, vol. xlviii.

² *Annals of the Cape Observatory*, vol. viii. part 2, and *Trans. Astron. Observatory of Yale University*, vol. i.

of $0''.76$, or is distant about $4\frac{1}{3}$ light-years. Its mass is independently known to be almost exactly equal to that of our Sun; and its spectrum being also identical with that of our Sun, we may reasonably assume that it appears to us of the same magnitude as would our Sun if removed to the distance of α_2 Centauri.

But the average star of the same apparent magnitude as α_2 Centauri was found to have a parallax of only $0''.10$, so that either α_2 Centauri or our Sun, if removed to a distance equal to that of the average fixed star of the first magnitude would appear to us but little brighter than a star of the fifth magnitude.

Again, there is a star of only $8\frac{1}{2}$ magnitude¹ which has the remarkable annual proper motion of nearly $8\frac{3}{4}$ seconds of arc—one of those so-called runaway stars—which moves with a velocity of 80 miles per second at right angles to the line of sight (we do not know with what velocity in the line of sight). It is at about the same distance from us as Sirius, but it emits but one ten-thousandth part of the light energy of that brilliant star. Sirius itself emits about thirty times the light-energy of our Sun, but it in turn sinks into insignificance when compared with the giant Canopus, which emits at least 10,000 times the light-energy of our Sun.

Truly 'one star differs from another star in glory.' Proper motion rather than apparent brightness is the truer indication of a star's probable proximity to the Sun. Every star of considerable proper motion yet examined has proved to have a measurable parallax.

This fact at once suggests the idea, Why should not the apparent parallactic motions of the stars, as produced by the Sun's motion in space, be utilised as a means of determining stellar parallax?

Secular Parallactic Motion of Stars.

The strength of such determinations, unlike those made by the method of annual parallax, would grow with time. It is true that the process cannot be applied to the determination of the parallax of individual stars, because the peculiar motion of a particular star cannot be separated from that part of its apparent motion which is due to parallactic displacement. But what we specially want is not to ascertain the parallax of the individual star, but the mean parallax of a particular group or class of stars, and for this research the method is specially applicable, provided we may assume that the peculiar motions are distributed at random, so that they have no systematic tendency in any direction; in other words, that the centre of gravity of any extensive group of stars will remain fixed in space.

This assumption is, of course, but a working hypothesis, and one which from the paper on star-streaming communicated by Professor Kapteyn of Groningen to the Johannesburg meeting of the Association two years ago we already know to be inexact.² Kapteyn's results were quite recently

¹ Gould's Zones, Vh 243.

² *Rep. Brit. Assoc.*, 1905, p. 257.

confirmed in a remarkable way by Eddington,¹ using independent material discussed by a new and elegant method. Both results showed that, at least for extensive parts of space, there are a nearly equal number of stars moving in exactly opposite directions. The assumption, then, that the mean of the peculiar motions is zero may, at least for these parts of space, be still regarded as a good working hypothesis.

Adopting an approximate position of the apex of the solar motion, Kapteyn resolved the observed proper motions of the Bradley stars into two components, viz, one in the plane of the great circle passing through the star and the apex, the other at right angles to that plane.² The former component obviously includes the whole of the parallax motion; the latter is independent of it, and is due entirely to the real motions of the stars themselves. From the former the mean parallax motion of the group is derived, and from the combination of the two components, the relation of velocity of the Sun's motion to that of the mean velocity of the stars of the group.

As the distance of any group of stars found by the parallax motion is expressed as a unit in terms of the Sun's yearly motion through space, the velocity of this motion is one of the fundamental quantities to be determined. If the mean parallax of any sufficiently extensive group or class of stars was known we should have at once means for a direct determination of the velocity of the Sun's motion in space; or if, on the other hand, we can by independent methods determine the Sun's velocity, then the mean parallax of any group of stars can be determined.

Determination of Stellar Motion in the Line of Sight.

Science owes to Sir William Huggins the application of Doppler's principle to the determination of the velocity of star-motion in the line of sight. The method is now so well known, and such an admirable account of its theory and practical development was given by its distinguished inventor from this Chair at the Cardiff meeting in 1891, that further mention of that part of the matter seems unnecessary.

The Velocity of the Sun's Motion in Space.

If by this method the velocities in the line of sight of a sufficient number of stars situated near the apex and antapex of the solar motion could be determined, so that in the mean it could be assumed that their peculiar motions would disappear, we have at once a direct determination of the required velocity of the Sun's motion.

The material for this determination is gradually accumulating, and indeed much of it, already accumulated, is not yet published. But even with

¹ *Monthly Notices R.A.S.*, vol. lxvii. p. 34.

² *Publications Astron. Laboratory Groningen*, Nos. 7 and 9.

the comparatively scant material available, it now seems almost certain that the true value of the Sun's velocity lies between 18 and 20 kilometres per second ;¹ or, if we adopt the mean value, 19 kilometres per second, this would correspond almost exactly with a yearly motion of the Sun through space equal to four times the distance of the Sun from the Earth.

Thus the Sun's yearly motion being four times the Sun's distance, the parallactic motion of stars in which this motion is unforeshortened must be four times their parallax. How this number varies with the amount of foreshortening is of course readily calculated. The point is that from the mean parallactic motion of a group of stars we are now enabled to derive at once its mean parallax.

This research has been carried out by Kapteyn for stars of different magnitudes. It leads to the result that the parallax of stars differing *five* magnitudes does *not* differ in the proportion of one to ten, as would follow from the supposition of equal luminosity of stars throughout the universe, but only in the proportion of about one to five.²

The same method cannot be applied to groups of stars of different proper motions, and it is only by a somewhat indirect proof, and by calling in the aid of such reliable results of direct parallax determination as we possess, that the variation of parallax with proper motion could be satisfactorily dealt with.

The Mean Parallaxes of Stars of Different Magnitude and Proper Motion.

As a final result Kapteyn derived an empirical formula giving the average parallax for stars of different spectral types, and of any given magnitude and proper motion. This formula was published at Groningen in 1901.³ Within the past few months the results of researches on stellar parallax, made under the direction of Dr. Elkin, at the Astronomical Observatory of Yale University, during the past thirteen years,⁴ have been published, and they afford a most crucial and entirely independent check on the soundness of Kapteyn's conclusions.

In considering the comparison between the more or less theoretical results of Kapteyn and the practical determinations of Yale, we have to remember that Kapteyn's tables refer only to the means of groups of a large number of stars having on the average a specified magnitude and proper motion, whilst the latter are direct determinations affected by the accidental errors of the separate determinations and by such uncertainty as attaches to the unknown parallaxes of the comparison stars—parallaxes which we have supplied from Kapteyn's general tables.

The Yale results consist of the determination of the parallax of 173 stars, of which only ten had been previously known to Kapteyn and had

¹ *Kapteyn Ast. Nach.*, No. 3487, p. 108 ; and Campbell, *Astrophys. Journ.*, xiii. p. 80.

² *Astron. Nachrichten*, No. 3487, Table III. ; and *Ast. Journ.*, p. 566.

³ *Publications Astron. Laboratory Groningen*, No. 8, p. 24.

⁴ *Trans. Astron. Observatory of Yale Univ.*, vol. ii., part 1.

been utilised by him. Dividing these results into groups we get the following comparison :—

Comparison Groups arranged in order of Proper Motion.

No. of Stars	Proper Motion	Magnitude	Parallax		Yale—Kapteyn
			Yale	Kapteyn	
21	0 14	3·8	0 028	0'026	+ 0'002
39	0 49	6·3	·042	·055	— ·013
45	0·59	6 7	·068	·060	+ ·008
46	0 77	6 5	·047	·074	— ·027
22	1 50	6 2	·118	·124	— ·006

Groups arranged in order of Magnitude.

No. of Stars	Proper Motion	Magnitude	Parallax		Yale—Kapteyn
			Yale	Kapteyn	
10	0·61	0 8	0'103	0'110	— 0'007
29	·53	3 8	·076	·075	+ ·001
33	·63	5 6	·064	·070	— ·006
34	·73	6·7	·055	·070	— ·017
31	·68	7 6	025	·061	— ·036
36	·80	8 3	056	·082	— ·006

	No. of Stars	Proper Motion	Magnitude	Parallax		Yale—Kapteyn
				Yale	Kapteyn	
Spectral Type I.	13	0 42	4 0	0 076	0'076	0 000
„ „ II.	81	0·67	5·3	0·067	0 074	— 0 007

These results agree in a surprisingly satisfactory way, having regard to the comparatively small number of stars in each group and the great range of parallax which we know to exist amongst individual stars having the same magnitude and proper motion. In the mean perhaps the tabular parallaxes are in a minute degree too large, but we have unquestionable proof from this comparison that our knowledge of stellar distances now rests on a solid foundation.

The Distribution of Varieties of Luminosity of Stars.

But, besides the mean parallax of stars of a particular magnitude and proper motion, it is essential that we should know approximately what percentage of the stars of such a group have twice, three times, &c., the mean parallax of the group, and what percentage only one-half, one-third of that parallax, and so on. In principle, at least, this frequency-law may be obtained by means of the directly determined parallaxes. For the stars of which we have reliable determinations we can compare

these true parallaxes with the *mean* parallax of stars having corresponding magnitude and proper motion, and this comparison will lead to a knowledge of the frequency-law required. It is true that, owing to the scarcity of material at present available, the determination of the frequency-law is not so strong as may be desirable, but further improvement is simply a question of time and the augmentation of parallax-determination.

Adopting provisionally the frequency-law found in this way by Kapteyn,¹ we can localise all the stars in space down to about the ninth magnitude.

Take, for example, the stars of magnitude 5.5 to 6.5. There are about 4,800 of these stars in the whole sky. According to Auwers-Bradley, about $9\frac{1}{2}$ per cent. of these stars, or some 460 in all, have proper motions between $0''.04$ and $0''.05$. Now, according to Kapteyn's empiric formula, whose satisfactory agreement with the Yale results has just been shown, the mean parallax of such stars is almost exactly $0''.01$. Further, according to his frequency-law, 29 per cent. of the stars have parallaxes between the *mean* value and double the mean value; 6 per cent. have parallaxes between twice and three times the mean value; $1\frac{1}{2}$ per cent. between three and four times the mean value. Therefore of our 460 stars 133 will have parallaxes between $0''.01$ and $0''.02$, twenty-eight between $0''.02$ and $0''.03$, seven between $0''.03$ and $0''.04$, and so on.

Localising in the same way the stars of the sixth magnitude having other proper motions, and then treating the stars of the first magnitude, second magnitude, third magnitude, and so on to the ninth magnitude in the same way, we finally locate all these stars in space.²

It is true we have not localised the individual stars, but we know approximately and within certain limits of magnitude the number of stars at each distance from the Sun.

Thus the apparent brightness and the distance being known we have the means of determining the light-energy or absolute *luminosity* of the stars, *provided it can be assumed that light does not suffer any extinction in its passage through interstellar space.*

On this assumption Kapteyn was led to the following results, viz., that within a sphere the radius of which is 560 light-years (a distance which corresponds with that of the average star of the ninth magnitude) there will be found :—

1 star giving from 100,000			to 10,000 times the light of our Sun.		
26 stars	„	10,000	1,000	„	„
1,300	„	1,000	100	„	„
22,000	„	100	10	„	„
140,000	„	10	1	„	„
430,000	„	1	0.1	„	„
650,000	„	0.1	0.01	„	„

¹ *Publications Astron. Lab. Groningen*, No. 8, p. 23.

² *Ibid.*, No. 11, Table II.

*The Density of Stellar Distribution at Different Distances from
our Sun.*

Consider, lastly, the distribution of stellar density, that is, the number of stars contained in the unit of volume.

We cannot determine *absolute* star-density, because, for example, some of the stars which we know from their measured parallaxes to be comparatively near to us are in themselves so little luminous that if removed to even a few light-years greater distance they would appear fainter than the ninth magnitude, and so fall below the magnitude at which our data at present stop.

But if we assume that intrinsically faint and bright stars are distributed in the same proportion in space, it will be evident that the comparative richness of stars in any part of the system will be the same as the comparative richness of the same part of the system in stars of a particular luminosity. Therefore, as we have already found the arrangement in space of the stars of different degrees of luminosity, and consequently their number at different distances from the Sun, we must also be able to determine their relative density for these different distances.

Kapteyn finds in this way that, starting from the Sun, the star-density (*i.e.*, the number of stars per unit volume of space) is pretty constant till we reach a distance of some 200 light-years. Thence the density gradually diminishes till, at about 2,500 light-years, it is only about *one-fifth* of the density in the neighbourhood of the Sun.¹ This conclusion must, however, be regarded as uncertain until we have by independent means been enabled to estimate the absorption of light in its course through interstellar space, and obtained proof that the ratio of intrinsically faint to bright stars is constant throughout the universe.

Thus far Kapteyn's researches deal with the stellar universe as a whole; the results, therefore, represent only the *mean* conditions of the system. The further development of our knowledge demands a like study applied to the several portions of the universe separately. This will require much more extensive material than we at present possess.

As a first further approximation the investigation will have to be applied separately to the Milky Way and the parts of the sky of higher galactic latitude. The velocity and direction of the Sun's motion in space may certainly be treated as constants for many centuries to come, and these constants may be separately determined from groups of stars of various regions, various magnitudes, various proper motions, and various spectral types. If these constants as thus separately determined are different, the differences which are not attributable to errors of observation must be due to a common velocity or direction of motion of the group or class of star to which the Sun's velocity or direction is referred. Thus, for example, the Sun's velocity as determined by spectroscopic observations

¹ *Publications Astron. Lab. Groningen*, No. 11.

of motion in the line of sight, appears to be sensibly smaller than that derived from fainter stars. The explanation appears to be that certain of the brighter stars form part of a cluster or group of which the Sun is a member, and these stars tend to some extent to travel together. For these researches the existing material, especially that of the determination of velocities in the line of sight, is far too scanty.

Kapteyn has found that stars whose proper motions exceed $0''.05$ are not more numerous in the Milky Way than in other parts of the sky;¹ in other words, if only the stars having proper motions of $0''.05$ or upwards were mapped there would be no aggregation of stars showing the existence of a Milky Way.

The proper motions of stars of the second spectral type are, as a rule, considerably larger than those of the first type; but Kapteyn comes to the conclusion that this difference does not mean a real difference of velocity, but only that the second-type stars have a smaller luminosity, the mean difference between the two types amounting to $2\frac{1}{2}$ magnitudes.²

The Future Course of Research.

In the last Address delivered from this Chair on an astronomical subject, Sir William Huggins, in 1891, dealt so fully with the chemistry of the stars that it seemed fitting on the present occasion to consider more especially the problem of their motion and distribution in space, as it is in this direction that the most striking advances in our knowledge have recently been made. It is true that since 1891 great advances have also been made in our detailed knowledge of the chemistry of the Sun and stars. The methods of astro-spectrography have been greatly improved, the precision of the determination of motion in the line of sight greatly enhanced, and many discoveries made of those close double stars, ordinarily termed spectroscopic doubles, the study of which seems destined to throw illustrative light upon the probable history of the development of systems from the original nebular condition to that of more permanent systems.

But the limitations of available time prevent me from entering more fully into this tempting field, more especially as it seems desirable, in the light of what has been said, to indicate the directions in which some of the astronomical work of the future may be most properly systematised. There are two aspects from which this question may be viewed. The first is the more or less immediate extension of knowledge or discovery; the second the fulfilment of our duty, as astronomers, to future generations. These two aspects should never be entirely separated. The first, as it opens out new vistas of research and improved methods of work, must often serve as a guide to the objects of the second. But the second is to the astronomer the supreme duty, viz., to secure for future generations those data the value of which grows by time.

¹ *Verh. Kn. Akad. Amsterdam*, January 1893.

² *Ibid.*, April 1892.

As the result of the Congress of Astronomers held at Paris in 1887 some sixteen of the principal observatories in the world are engaged, as is well known, in the laborious task, not only of photographing the heavens, but of measuring these photographs and publishing the *relative* positions of the stars on the plates down to the eleventh magnitude. A century hence this great work will have to be repeated, and then, if we of the present day have done our duty thoroughly, our successors will have the data for an infinitely more complete and thorough discussion of the motions of the sidereal system than any that can be attempted to-day. But there is still needed the accurate meridian observation of some eight or ten stars on each photographic plate, so as to permit the conversion of the *relative* star-places on the plate into *absolute* star-places in the heavens. It is true that some of the astronomers have already made these observations for the reference stars of the zones which they have undertaken. But this seems to be hardly enough. In order to co-ordinate these zones, as well as to give an accuracy to the *absolute* positions of the reference stars corresponding with that of the *relative* positions, it is desirable that this should be done for *all* the reference stars in the sky by several observatories. The observations of well-distributed stars by Kustner at Bonn present an admirable instance of the manner in which the work should be done. Several observatories in each hemisphere should devote themselves to this work, employing the same or other equally efficient means for the elimination of sources of systematic error depending on magnitude, &c., and it is of far more importance that we should have, say, two or three observations of each star at three different observatories than two or three times as many observations of each star made at a single observatory.

The southern cannot boast of a richness of instrumental and personal equipment comparable with that of the northern hemisphere, and consequently one welcomes with enthusiasm the proposal on the part of the Carnegie Institute to establish a meridian observatory in a suitable situation in the southern hemisphere. Such an observatory, energetically worked, with due attention to all necessary precautions for the exclusion of systematic errors, would conduce more than anything else to remedy in some degree that want of balance of astronomical effort in the two hemispheres to which allusion has already been made. But in designing the programme of the work it should be borne in mind that the proper duty of the meridian instrument in the present day is no longer to determine the positions of all stars down to a given order of magnitude, but to determine the positions of stars which are geometrically best situated and of the most suitable magnitude for measurement on photographic plates, and to connect these with the fundamental stars. For this purpose the working list of such an observatory should include only the fundamental stars and the stars which have been used as reference stars for the photographic plates.

Such a task undertaken by the Carnegie Observatory, by the Cape,

and if possible by another observatory in the southern hemisphere, and by three observatories in the northern, would be regarded by astronomers of the future as the most valuable contribution that could be made to astronomy of the present day. Taken in conjunction with the astrographic survey of the heavens now so far advanced, it is an opportunity that if lost can never be made good ; a work that would grow in value year by year as time rolls on, and one that would ever be remembered with gratitude by the astronomers of the future.

But for the solution of the riddle of the universe much more is required. Besides the proper motions, which would be derived from the data just described, we need for an ideal solution to know the velocity in the line of sight, the parallax, the magnitude, and the spectrum-type of every star.

The broad distinction between these latter data and the determination of proper motion is this, that whereas the observations for proper motion increase in value as the square of their age, those for velocity in the line of sight, parallax, magnitude, and type of spectrum may, for the broader purposes of cosmical research, be made at any time without loss of value. We should therefore be most careful not to sacrifice the interests of the future by immediate neglect of the former for the latter lines of research. The point is that those observatories which undertake this meridian work should set about it with the least possible delay, and prosecute the programme to the end with all possible zeal. Three observatories in each hemisphere should be sufficient ; the quality of the work should be of the best, and quality should not be sacrificed for speed of work.

But the sole prosecution of routine labour, however high the ultimate object, would hardly be a healthy condition for the astronomy of the immediate future. The sense of progress is essential to healthy growth, the desire to know must in some measure be gratified. We have to test the work that we have done in order to be sure that we are working on the right lines, and new facts, new discoveries, are the best incentives to work.

For these reasons Kapteyn, in consultation with his colleagues in different parts of the world, has proposed a scheme of research which is designed to afford within a comparatively limited time a great augmentation of our knowledge. The principle on which his programme is based is that adequate data as to the proper motions, parallaxes, magnitudes, and the type of spectrum of stars situated in limited but symmetrically distributed areas of the sky, will suffice to determine many of the broader facts of the constitution of the universe. His proposals and methods are known to astronomers and need not therefore be here repeated. In all respects save one these proposals are practical and adequate, and the required co-operation may be said to be already secured—the exception is that of the determination of motion in the line of sight.

All present experience goes to show that there is no known satisfactory method of determining radial velocity of stars by wholesale methods, but

that such velocities must be determined star by star. For the fainter stars huge telescopes and spectroscopes of comparatively low dispersion must be employed. On this account there is great need in both hemispheres of a huge reflecting telescope—six to eight feet in aperture—devoted almost exclusively to this research. Such a telescope is already in preparation at Mount Wilson, in America, for use in the northern hemisphere. Let us hope that Professor Pickering's appeal for a large reflector to be mounted in the southern hemisphere will meet with an adequate response, and that it will be devoted there to this all-important work.

Conclusion.

The ancient philosophers were confident in the adequacy of their intellectual powers alone to determine the laws of human thought and regulate the actions of their fellow men, and they did not hesitate to employ the same unsupported means for the solution of the riddle of the universe. Every school of philosophy was agreed that some object which they could see was a fixed centre of the universe, and the battle was fought as to what that centre was. The absence of facts, their entire ignorance of methods of exact measurement, did not daunt them, and the question furnished them a subject of dispute and fruitless occupation for twenty-five centuries.

But astronomers now recognise that Bradley's meridian observations at Greenwich, made only 150 years ago, have contributed more to the advancement of sidereal astronomy than all the speculations of preceding centuries. They have learned the lesson that human knowledge in the slowly developing phenomena of sidereal astronomy must be content to progress by the accumulating labours of successive generations of men ; that progress will be measured for generations yet to come more by the amount of honest, well directed, and systematically discussed observation than by the most brilliant speculation ; and that, in observation, concentrated systematic effort on a special thoughtfully selected problem will be of more avail than the most brilliant but disconnected work.

By these means we shall learn more and more of the wonders that surround us, and recognise our limitations when measurement and facts fail us.

Huggins's spectroscope has shown that many nebulae are not stars at all ; that many well-condensed nebulae, as well as vast patches of nebulous light in the sky, are but inchoate masses of luminous gas. Evidence upon evidence has accumulated to show that such nebulae consist of the matter out of which stars (*i.e.*, suns) have been and are being evolved. The different types of star spectra form such a complete and gradual sequence (from simple spectra resembling those of nebulae onwards through types of gradually increasing complexity) as to suggest that we have before us, written in the cryptograms of these spectra, the complete story of the evolution of suns from the inchoate nebula onwards to the most active

sun (like our own), and then downward to the almost heatless and invisible ball. The period during which human life has existed on our globe is probably too short—even if our first parents had begun the work—to afford observational proof of such a cycle of change in any particular star ; but the fact of such evolution, with the evidence before us, can hardly be doubted. I most fully believe that, when the modifications of terrestrial spectra under sufficiently varied conditions of temperature, pressure, and environment have been further studied, this conclusion will be greatly strengthened. But in this study we must have regard also to the spectra of the stars themselves. The stars are the crucibles of the Creator. There we see matter under conditions of temperature and pressure and environment, the variety of which we cannot hope to emulate in our laboratories, and on a scale of magnitude beside which the proportion of our greatest experiment is less than that of the drop to the ocean. The spectroscopic astronomer has to thank the physicist and the chemist for the foundation of his science, but the time is coming—we almost see it now—when the astronomer will repay the debt by his continuing contributions to the very fundamenta of chemical science.

By patient, long-continued labour in the minute sifting of numerical results, the grand discovery has been made that a great part of space, so far as we have visible knowledge of it, is occupied by two majestic streams of stars travelling in opposite directions. Accurate and minute measurement has given us some certain knowledge as to the distances of the stars within a certain limited portion of space, and in the cryptograms of their spectra has been deciphered the amazing truth that the stars of both streams are alike in design, alike in chemical constitution, and alike in process of development.

But whence have come the two vast streams of matter out of which have been evolved these stars that now move through space in such majestic procession ?

The hundreds of millions of stars that comprise these streams, are they the sole ponderable occupants of space ? However vast may be the system to which they belong, that system itself is but a speck in illimitable space ; may it not be but one of millions of such systems that pervade the infinite ?

We do not know.

‘Canst thou by searching find out God ? canst thou find out the Almighty unto perfection ?’

British Association for the Advancement of Science.

DUBLIN, 1908.

ADDRESS

BY

FRANCIS DARWIN, M.A., PH.D., LL.D., F.R.S.,
PRESIDENT.

BEFORE entering on the subject of my Address, I may be allowed to refer to the loss which the British Association has sustained in the death of Lord Kelvin. He joined the Association in 1847, and had been for more than fifty years a familiar figure at our meetings. This is not the occasion to speak of his work in the world or of what he was to his friends, but rather of his influence on those who were personally unknown to him. It seems to me characteristic of him that something of his vigour and of his personal charm was felt far beyond the circle of his intimate associates, and many men and women who never exchanged a word with Lord Kelvin, and are in outer darkness as to his researches, will miss his genial presence and feel themselves the poorer to-day. By the death of Sir John Evans the Association is deprived of another faithful friend. He presided at Toronto in 1897, and since he joined the Association in 1861 had been a regular attendant at our meetings. The absence of his cheerful personality and the loss of his wise counsels will be widely felt.

May I be permitted one other digression before I come to my subject? There has not been a Botanical President of the British Association since the Norwich meeting forty years ago, when Sir Joseph Hooker was in the chair, and in 'eloquent and felicitous words' (to quote my father's letter) spoke in defence of the doctrine of evolution. I am sure that every member of this Association will be glad to be reminded that Sir Joseph Hooker is, happily, still working at the subject that his lifelong labours have so greatly advanced, and of which he has long been recognised as the honoured chief and leader.

You will perhaps expect me to give a retrospect of the progress of evolution during the fifty years that have elapsed since July 1, 1858, when the doctrine of the origin of species by means of natural selection was

made known to the world in the words of Mr. Darwin and Mr. Wallace. This would be a gigantic task, for which I am quite unfitted. It seems to me, moreover, that the first duty of your President is to speak on matters to which his own researches have contributed. My work—such as it is—deals with the movements of plants, and it is with this subject that I shall begin. I want to give you a general idea of how the changes going on in the environment act as stimuli and compel plants to execute certain movements. Then I shall show that what is true of those temporary changes of shape we describe as movements is also true of the permanent alterations known as morphological.

I shall insist that, if the study of movement includes the problem of stimulus and reaction, morphological change must be investigated from the same point of view. In fact, that these two departments of inquiry must be classed together, and this, as we shall see, has some important results—namely, that the dim beginnings of habit or unconscious memory that we find in the movements of plants and animals must find a place in morphology; and inasmuch as a striking instance of correlated morphological changes is to be found in the development of the adult from the ovum, I shall take this ontogenetic series and attempt to show you that here also something equivalent to memory or habit reigns.

Many attempts have been made to connect in this way the phenomena of memory and inheritance, and I shall ask you to listen to one more such attempt, even though I am forced to appear as a champion of what some of you consider a lost cause—the doctrine of the inheritance of acquired characters.

Movement.

In his book on 'The Power of Movement in Plants' (1880)¹ my father wrote that 'it is impossible not to be struck with the resemblance between the foregoing movements of plants and many of the actions performed unconsciously by the lower animals.' In the previous year Sachs² had in like manner called attention to the essential resemblance between the irritability of plants and animals. I give these statements first because of their simplicity and directness; but it must not be forgotten that before this Pfeffer³ had begun to lay down the principles of what is now known as *Reizphysiologie*, or the physiology of stimulus, for which he and his pupils have done so much.

The words of Darwin which I have quoted afford an example of the way in which science returns to the obvious. Here we find revived, in a rational form, the point of view of the child or of the writer of fairy stories. We do not go so far as the child; we know that flowers do not talk or walk; but the fact that plants must be classed with animals as regards their manner of reaction to stimuli has now become almost a

P. 571.

² *Arbeiten*, ii. 1879, p. 282.

Osmotische Untersuchungen, 1877, p. 202.

commonplace of physiology. And inasmuch as we ourselves are animals, this conception gives us a certain insight into the reactions of plants which we should not otherwise possess. This is, I allow, a very dangerous tendency, leading to anthropomorphism, one of the seven deadly sins of science. Nevertheless, it is one that must be used unless the great mass of knowledge accumulated by psychologists is to be forbidden ground to the physiologist.

Jennings¹ has admirably expressed the point of view from which we ought to deal with the behaviour of the simpler organisms. He points out that we must study their movements in a strictly objective manner : that the same point of view must be applied to man, and that any resemblances between the two sets of phenomena are not only an allowable but a necessary aid to research.

What, then, are the essential characters of stimuli and of the reactions which they call forth in living organisms? Pfeffer has stated this in the most objective way. An organism is a machine which can be set going by touching a spring or trigger of some kind ; a machine in which energy can be set free by some kind of releasing mechanism. Here we have a model of at least some of the features of reaction to stimulation.

The energy of the cause is generally out of all proportion to the effect, *i.e.*, a small stimulus produces a big reaction. The specific character of the result depends on the structure of the machine rather than on the character of the stimulus. The trigger of a gun may be pulled in a variety of different ways without affecting the character of the explosion. Just in the same way a plant may be made to curve by altering its angle to the vertical, by lateral illumination, by chemical agency, and so forth ; the curvature is of the same nature in all cases, the release-action differs. One of those chains of wooden bricks in which each knocks over the next may be set in action by a touch, by throwing a ball, by an erring dog, in short by anything that upsets the equilibrium of brick No. 1 ; but the really important part of the game, the way in which the wave of falling bricks passes like a prairie fire round a group of Noah's Ark animals, or by a bridge over its own dead body and returns to the starting-point, &c.—these are the result of the magnificent structure of the thing as a whole, and the upset of brick No. 1 seems a small thing in comparison.

For myself I see no reason why the term *stimulus* should not be used in relation to the action of mechanisms in general ; but by a convention which it is well to respect, *stimulation* is confined to the protoplasmic machinery of living organisms.

The want of proportion between the stimulus and the reply, or, as it has been expressed, the unexpectedness of the result of a given stimulus, is a striking feature in the phenomena of reaction. That this should be so need not surprise us. We can, as a rule, only know the stimulus and

the response, while the intermediate processes of the mechanism are hidden in the secret life of protoplasm. We might, however, have guessed that big changes would result from small stimuli, since it is clear that the success of an organism in the world must depend partly at least on its being highly sensitive to changes in its surroundings. This is the adaptive side of the fundamental fact that living protoplasm is a highly unstable body. Here I may say one word about the adaptation as treated in the *Origin of Species*. It is the present fashion to minimise or deny altogether the importance of natural selection. I do not propose to enter into this subject; I am convinced that the inherent strength of the doctrine will insure its final victory over the present anti-Darwinian stream of criticism. From the Darwinian point of view it would be a remarkable fact if the reactions of organisms to natural stimuli were not adaptive. That they should be so, as they undoubtedly are, is not surprising. But just now I only call attention to the adaptive character of reactions from a descriptive point of view.

Hitherto I have implied the existence of a general character in stimulation without actually naming it; I mean the indirectness of the result. This is the point of view of Dutrochet, who in 1824 said that the environment suggests but does not directly cause the reaction. It is not easy to make clear in a few words the conception of indirectness. Pfeffer¹ employs the word *induction*, and holds that external stimuli act by producing internal change, such changes being the link between stimulus and reaction. It may seem, at first sight, that we do not gain much by this supposition; but since these changes may be more or less enduring, we gain at least the conception of *after effect* as a quality of stimulation. What are known as *spontaneous* actions must be considered as due to internal changes of unknown origin.

It may be said that in speaking of the 'indirectness' of the response to stimuli we are merely expressing in other words the conception of release-action; that the explosion of a machine is an indirect reply to the touch on the trigger. This is doubtless true, but we possibly lose something if we attempt to compress the whole problem into the truism that the organism behaves as it does because it has a certain structure. The quality of indirectness is far more characteristic of an organism than of a machine, and to keep it in mind is more illuminating than a slavish adherence to the analogy of a machine. The reaction of an organism depends on its past history; but, it may be answered, this is also true of a machine the action of which depends on how it was made, and in a less degree on the treatment it has received during use. But in living things this last feature in behaviour is far more striking, and in the higher organisms past experience is all-important in deciding the response to stimulus. The organism is a plastic machine profoundly affected in structure by its own action, and the unknown process intervening between

¹ *Physiology*, Engl. edit., i. p. 11.

stimulus and reaction (on which the indirectness of the response depends) must have the fullest value allowed it as a characteristic of living creatures.

For the zoological side of biology a view similar to that of Pfeffer has been clearly stated by Jennings¹ in his admirable studies on the behaviour of infusoria, rotifers, &c. He advances strong arguments against the theories of Loeb and others, according to which the stimulus acts directly on the organs of movement; a point of view which was formerly held by botanists, but has since given place to the conception of the stimulation acting on the organism as a whole. Unfortunately for botanists these movements are by the zoologists called *tropisms*, and are thus liable to be confused with the geotropism, heliotropism, &c., of plants: to these movements, which are not considered by botanists to be due to direct action of stimuli, Loeb's assumptions do not seem to be applicable.

Jennings's position is that we must take into consideration what he calls 'physiological state, *i.e.*, "the varying internal physiological conditions of the organism, as distinguished from permanent anatomical conditions."' Though he does not claim novelty for his view, I am not aware that it has ever been so well stated. External stimuli are supposed to act by altering this physiological state; that is, the organism is temporarily transformed into what, judged by its reactions, is practically a different creature.

This may be illustrated by the behaviour of *Stentor*, one of the fixed infusoria.² If a fine jet of water is directed against the disc of the creature, it contracts 'like a flash' into its tube. In about half a minute it expands again and the cilia resume their activity. Now we cause the current to act again upon the disc. This time the *Stentor* does not contract, which proves that the animal has been in some way changed by the first stimulus. This is a simple example of 'physiological state.' When the *Stentor* was at rest, before it received the first current of water, it was in state 1, the stimulus changed state 1 into state 2, to which contraction is the reaction. When again stimulated it passed into state 3, which does not produce contraction.

We cannot prove that the contraction which occurred when the *Stentor* was first stimulated was due to a change of state. But it is a fair deduction from the result of the whole experiment, for after the original reaction the creature is undoubtedly in a changed state, since it no longer reacts in the same way to a repetition of the original stimulus.

Jennings points out that, as in the case of plants, spontaneous acts are brought about when the physiological state is changed by unknown causes, whereas in other cases we can point to an external agency by which the same result is effected.

¹ H. S. Jennings, *Contributions to the Study of the Behavior of the Lower Organisms*. Carnegie Institution, 1904, p. 111.

² Jennings, *Behavior of the Lower Organisms*, 1906, p. 170.

Morphological Changes.

Let us pass on to the consideration of the permanent or morphological changes and the stimuli by which they are produced, a subject to which, in recent years, many workers have devoted themselves. I need only mention the names of Vochting, Goebel, and Klebs among botanists, and those of Loeb, Herbst, and Driesch among zoologists, to remind you of the type of research to which I refer.

These morphological alterations produced by changes in environment have been brought under the rubric of reaction to stimulation, and must be considered as essentially similar to the class of temporary movements of which I have spoken.

The very first stage in development may be determined by a purely external stimulus. Thus the position of the first cell-wall in the developing spore of *Equisetum* is determined by the direction of incident light.¹ In the same way the direction of light settles the plane of symmetry of *Marchantia* as it develops from the gemma.² But the more interesting cases are those where the presence or absence of a stimulus makes an elaborate structural difference in the organism. Thus, as Stahl³ has shown, beech leaves developed in the deep shade of the middle of the tree are so different in structure from leaves grown in full sunlight that they would unhesitatingly be described as belonging to different species. Another well-known case is the development of the scale-leaves on the rhizome of *Circea* into the foliage leaves under the action of light.⁴

The power which the experimenter has over the lower plants is shown by Klebs, who kept *Saprolegnia mixta*, a fungus found on dead flies, in uninterrupted vegetative growth for six years; while by removing a fragment of the plant and cultivating it in other conditions the reproductive organs could at any time be made to appear.⁵

Chlamydomonas media, a unicellular green alga, when grown in a 0.4 per cent. nutrient solution continues to increase by simple division, but conjugating gametes are formed in a few days if the plant is placed in pure water and kept in bright light.⁶ Numberless other cases could be given of the regulation of form in the lower organisms. Thus *Sporodinia* grown on peptone-gelatine produces sporangiferous hypha, but on sugar zygotes are formed. Again, *Protosiphon botryoides*, if grown on damp clay, can most readily be made to produce spores by tranference to water either in light or in darkness. But for the same plant cultivated in Knop's solution the end can best be obtained by placing the culture in the dark.⁷ Still these instances of the regulation of reproduction are not so interesting from our point of view as some of Klebs' later results.⁸ Thus he has shown that the colour of the flower of *Campanula trachelium*

Stahl, *Ber. d. Bot. Ges.*, 1885, p. 334.

¹ *Jenaische Zeitschr.*, 1883, p. 162.

² *Willkürliche Entwick.*, p. 27.

Biol. Centralbl., 1904, pp. 451-3.

³ Pfeffer, in Sachs' *Arbeiten*, i. p. 92.

⁴ Goebel in *Bot. Zeitung*, 1880.

⁵ Klebs, *Bedingungen*, 1896, p. 430.

⁶ *Jahrb. f. wiss. Bot.*, xlii. 1906, p. 162.

can be changed from blue to white and back again to blue by varying the conditions under which the plant is cultivated. Again, with *Sempervivum*¹ he has been able to produce striking results—*e.g.*, the formation of apetalous flowers with one instead of two rows of stamens. Diminution in the number of stamens is a common occurrence in his experimental plants, and absolute loss of these organs also occurs. Many other abnormalities were induced, both in the stamens and in other parts of the flowers.

There is nothing new in the character of these facts;² what has been brought to light (principally by the work of Klebs) is the *degree* to which ontogeny is controllable. We are so much in the habit of thinking of the stable element in ontogeny that the work of Klebs strikes us with something of a shock. Most people would allow that change of form is ultimately referable to changed conditions, but many of us were not prepared to learn the great importance of external stimuli in ontogeny.

Klebs begins by assuming that every species has a definite *specific structure*, which he compares to chemical character. Just as a substance such as sulphur may assume different forms under different treatment, so he assumes that the specific structure of a plant has certain potentialities which may be brought to light by appropriate stimuli. He divides the agencies affecting the structure into external and internal conditions, the external being supposed to act by causing alterations in the internal conditions.

It will be seen that the scheme is broadly the same as that of Pfeffer for the case of the movement and other temporary reactions. The internal conditions of Klebs correspond also to the 'physiological state' of Jennings.

From what has gone before, it will be seen that the current conception of stimulus³ is practically identical whether we look at the

¹ *Abhandl. Naturforsch. Ges. zu Halle*, xxv., 1906, pp. 31, 34, &c.

² See the great collection of facts illustrating the 'direct and definite action of the external conditions of life' in *Variation of Animals and Plants*, ii. 271

³ With regard to the terminology of stimulation, I believe that it would greatly simplify matters if our classification of causal conditions could be based on the relation of the nucleus to the rest of the cell. But our knowledge does not at present allow of more than a tentative statement of such a scheme. It is now widely believed that the nucleus is the bearer of the qualities transmitted from generation to generation, and the regulator of ontogeny. May we not therefore consider it probable that the nucleus plays in the cell the part of a central nervous system? In plants there is evidence that the ectoplasm is the sensitive region, and, in fact, plays the part of the cell's sense-organ. The change that occurs in the growth of a cell, as a response to stimulus, would on this scheme be a reflex action dependent for its character on the structure of the nucleus. The 'indirectness' of stimulation would then depend on the reception by the nucleus of the excitation set up in the ectoplasm, and the secondary excitation reflected from the nucleus, leading to certain changes in the growth of the cell.

If the nucleus be the bearer of the past history of the individual, the scheme here sketched would accord with the adaptive character of normal reactions and would

phenomena of movement or those of structure. If this is allowable—and the weight of evidence is strongly in its favour—a conclusion of some interest follows.

If we reconsider what I have called the indirectness of stimulation, we shall see that it has a wider bearing than is at first obvious. The 'internal condition' or 'physiological state' is a factor in the regulation of the organism's action, and it is a factor which owes its character to external agencies which may no longer exist.

The fact that stimuli are not momentary in effect but leave a trace of themselves on the organism is in fact the physical basis of the phenomena grouped under memory in its widest sense as indicating that action is regulated by past experience. Jennings¹ remarks: 'In the higher animals, and especially in man, the essential features in behaviour depend very largely on the history of the individual; in other words, upon the present physiological condition of the individual, as determined by the stimuli it has received and the reactions it has performed. But in this respect the higher animals do not differ in principle, but only in degree, from the lower organisms. . . .' I venture to believe that this is true of plants as well as of animals, and that it is further broadly true not only of physiological behaviour, but of the changes that are classed as morphological.

Semon in his interesting book, *Die Mneme*,² has used the word *Engram* for the trace or record of a stimulus left on the organism. In this sense we may say that the internal conditions of Pfeffer, the physiological states of Jennings, and the internal conditions of Klebs are, broadly speaking, *Engrams*. The authors of these theories may perhaps object to this sweeping statement, but I venture to think it is broadly true.

The fact that in some cases we recognise the chemical or physical character of the internal conditions does not by any means prevent our ascribing a *mnemic* memory-like character to them, since they remain causal agencies built up by external conditions which have, or may have, ceased to exist. Memory will be none the less memory when we know something of the chemistry and physics of its neural concomitant.

fall into line with what we know of the regulation of actions in the higher organisms. Pfeffer (*Physiology of Plants*, Eng. trans., iii. 10) has briefly discussed the possibility of thus considering the nucleus as a reflex centre, and has pointed out difficulties in the way of accepting such a view as universally holding good. Delage (*L'Hérédité*, 2nd edit., 1903, p. 88) gives a good summary of the evidence which induces him to deny the mastery of the cell by the nucleus. Driesch, however (*Analytische Theorie der organischen Entwicklung*, 1894, p. 81), gives reasons for believing that the cytoplasm is the receptive region, while the nucleus is responsible for the reaction, and it is on this that he bases his earlier theory of ontogeny.

¹ P. 124 (1904).

² *Die Mneme, als erhaltendes Prinzip im Wechsel des organischen Geschehens*, von Richard Semon, 1^{te} Auflage, Leipzig 1904, 2^{te} Auflage, 1908. It is a pleasure to express my indebtedness to this work, as well as for the suggestions and criticisms which I owe to Professor Semon personally.

Habit illustrated by Movement.

In order to make my meaning plain as to the existence of a *ninemic* factor in the life of plants, I shall for the moment leave the morphological side of life and give an instance of habitual movement.

Sleeping plants are those in which the leaves assume at night a position markedly different from that shown by day. Thus the leaflets of the scarlet-runner (*Phaseolus*) are more or less horizontal by day and sink down at night. This change of position is known to be produced by the alternation of day and night. But this statement by no means exhausts the interest of the phenomenon. A sensitive photographic plate behaves differently in light and darkness; and so does a radiometer, which spins by day and rests at night.

If a sleeping-plant is placed in a dark room after it has gone to sleep at night, it will be found next morning in the light-position, and will again assume the nocturnal position as evening comes on. We have, in fact, what seems to be a habit built by the alternation of day and night. The plant normally drops its leaves at the stimulus of darkness and raises them at the stimulus of light. But here we see the leaves rising and falling in the absence of the accustomed stimulation. Since this change of position is not due to external conditions it must be the result of the internal conditions which habitually accompany the movement. This is the characteristic *par excellence* of habit—namely, a capacity, acquired by repetition, of reacting to a fraction of the original environment. We may express it in simpler language. When a series of actions are compelled to follow each other by applying a series of stimuli they become organically tied together, or *associated*, and follow each other automatically, even when the whole series of stimuli are not acting. Thus in the formation of habit *post hoc* comes to be equivalent to *propter hoc*. Action B automatically follows action A, because it has repeatedly been compelled to follow it.

This may be compared with Herbert Spencer's¹ description of an imaginary case, that of a simple aquatic animal which contracts its tentacles on their being touched by a fish or a bit of seaweed washed against it. If such a creature is also sensitive to light the circumstances under which contraction takes place will be made up of two stimuli—those of light and of contact—following each other in rapid succession. And, according to the above statement of the essential character of associative habit, it will result that the light-stimulus alone may suffice, and the animal will contract without being touched.

Jennings² has shown that the basis of memory by association exists in so low an organism as the infusorian *Stentor*. When the animal is stimulated by a jet of water containing carmine in suspension, a physiological state A is produced, which, however, does not immediately lead to a visible

¹ *Psychology*, 2nd edit, 1870, vol. i. p. 435.

² *Behavior of the Lower Organisms*, 1906, p. 289.

reaction. As the carmine stimulus is continued or repeated, state B is produced, to which the *Stentor* reacts by bending to one side. After several repetitions of the stimulus, state C is produced, to which the animal responds by reversing its ciliary movement, and C finally passes into D, which results in the *Stentor* contracting into its tube. The important thing is that after many repetitions of the above treatment the organism 'contracts at once as soon as the carmine comes in contact with it.' In other words, states B and C are apparently omitted, and A passes directly into D, *i.e.*, into the state which gives contraction as a reaction. Thus we have in an infusorian a case of short-circuiting precisely like the case which has been quoted from Herbert Spencer as illustrating association. But Jennings' case has the advantage of being based on actual observation. He generalises the result as the 'law of the resolution of physiological states' in the following words: 'The resolution of one physiological state into another becomes easier and more rapid after it has taken place a number of times.' He goes on to point out that the operation of this law is seen in the higher organisms, 'in the phenomena which we commonly call memory, association, habit-formation, and learning.'

In spite of this evidence of mnemic power in the simplest of organisms, objections will no doubt be made to the statement that association of engrams can occur in plants.

Pfeffer, whose authority none can question, accounts for the behaviour of sleeping plants principally on the more general ground that when any movement occurs in a plant there is a tendency for it to be followed by a reversal—a swing of the physiological pendulum in the other direction. Pfeffer¹ compares it to a released spring which makes several alternate movements before it settles down to equilibrium. But the fact that the return movements occur at the same time-intervals as the stimuli is obviously the striking feature of the case. If the pendulum-like swing always tended to occur naturally in a twelve hours' rhythm it would be a different matter. But Pfeffer has shown that a rhythm of six hours can equally well be built up. And the experiments of Miss Pertz and myself² show that a half-hourly or quarter-hourly rhythm can be produced by alternate geotropic stimulation.

We are indebted to Keeble³ for an interesting case of apparent habit among the lower animals. *Convoluta roscoffensis*, a minute wormlike creature found on the coast of Brittany, leads a life dependent on the ebb and flow of the sea. When the tide is out the *Convoluta* come to the surface, showing themselves in large green patches. As the rising tide begins to cover them they sink down into safer quarters. The remarkable fact is that when kept in an aquarium, and therefore removed from tidal

¹ See Pfeffer, *Abhandl. K. Sachs. Ges.*, Bd. xxx. 1907. It is impossible to do justice to Pfeffer's point of view in the above brief statement.

² *Annals of Botany*, 1892 and 1903.

³ Gamble and Keeble, *Q. J. Mic. Science*, xlvii. p. 401.

action, they continue for a short time to perform rhythmic movements in time with the tide.

Let us take a human habit, for instance that of a man who goes a walk every day and turns back at a given mile-post. This becomes habitual, so that he reverses his walk automatically when the limit is reached. It is no explanation of the fact that the stimulus which makes him start from home includes his return—that he has a mental return-ticket. Such explanation does not account for the point at which he turns, which as a matter of fact is the result of association. In the same way a man who goes to sleep will ultimately wake ; but the fact that he wakes at four in the morning depends on a habit built up by his being compelled to rise daily at that time. Even those who will deny that anything like association can occur in plants cannot deny that in the continuance of the nyctitropic rhythm in constant conditions we have, in plants, something which has general character of habit, *i.e.*, a rhythmic action depending on a rhythmic stimulus that has ceased to exist.

On the other hand, many will object that even the simplest form of association implies a nervous system. With regard to this objection it must be remembered that plants have two at least of the qualities characteristic of animals—namely, extreme sensitiveness to certain agencies and the power of transmitting stimuli from one part to another of the plant body. It is true that there is no central nervous system, nothing but a complex system of nuclei ; but these have some of the qualities of nerve cells, while intercommunicating protoplasmic threads may play the part of nerves. Spencer¹ bases the power of association on the fact that every discharge conveyed by a nerve ‘leaves it in a state for conveying a subsequent like discharge with less resistance.’ Is it not possible that the same thing may be as true of plants as it apparently is of infusoria ? We have seen reasons to suppose that the ‘internal conditions’ or ‘physiological states’ in plants are of the nature of engrams, or residual effects of external stimuli, and such engrams may become associated in the same way.

There is likely to be another objection to my assumption that a simple form of associated action occurs in plants—namely, that association implies consciousness. It is impossible to know whether or not plants are conscious ; but it is consistent with the doctrine of continuity that in all living things there is something psychic, and if we accept this point of view we must believe that in plants there exists a faint copy of what we know as consciousness in ourselves.²

I am told by psychologists that I must define my point of view. I am accused of occupying that unscientific position known as ‘sitting on the fence.’ It is said that, like other biologists, I try to pick out what suits my purpose from two opposite schools of thought—the psychological and the physiological.

¹ *Psychology*, 2nd edit., vol. i. p. 615.

² See James Ward, *Naturalism and Agnosticism*, vol. i., Lecture X.

What I claim is that, as regards reaction to environment, a plant and a man must be placed in the same great class, in spite of the obvious fact that as regards complexity of behaviour the difference between them is enormous. I am not a psychologist, and I am not bound to give an opinion as to how far the occurrence of definite actions in response to stimulus is a physiological and how far a psychological problem. I am told that I have no right to assume the neural series of changes to be the cause of the psychological series, though I am allowed to say that neural changes are the universal concomitants of psychological change. This seems to me, in my ignorance, an unsatisfactory position. I find myself obliged to believe that the mnemonic quality in all living things (which is proved to exist by direct experiment) must depend on the physical changes in protoplasm, and that it is therefore permissible to use these changes as a notation in which the phenomena of habit may be expressed.

Habit illustrated by Morphology.

We have hitherto been considering the mnemonic quality of movements ; but, as I have attempted to show, morphological changes are reactions to stimulation of the same kind as these temporary changes. It is indeed from the morphological reactions of living things that the most striking cases of habit are, in my opinion, to be found.

The development of the individual from the germ-cell takes place by a series of stages of cell-division and growth, each stage apparently serving as a stimulus to the next, each unit following its predecessor like the movements linked together in an habitual action performed by an animal.

My view is that the rhythm of ontogeny is actually and literally a habit. It undoubtedly has the feature which I have described as pre-eminently characteristic of habit, viz., an automatic quality which is seen in the performance of a series of actions in the absence of the complete series of stimuli to which they (the stages of ontogeny) were originally due. This is the chief point on which I wish to insist—I mean that the resemblance between ontogeny and habit is not merely superficial, but deeply seated. It was with this conclusion in view that I dwelt, at the risk of being tedious, on the fact that memory has its place in the morphological as well as in the temporary reactions of living things. It cannot be denied that the ontogenetic rhythm has the two qualities observable in habit—namely, a certain degree of fixity or automaticity, and also a certain variability. A habit is not irrevocably fixed, but may be altered in various ways. Parts of it may be forgotten or new links may be added to it. In ontogeny the fixity is especially observable in the earlier, the variability in the later, stages. Mr. Darwin has pointed out that ‘on the view that species are only strongly marked and fixed varieties, we might expect often to find them still continuing to vary in those parts of their structure which have varied within a moderately recent period.’ These remarks are in explanation of the ‘notorious’ fact

that specific are more variable than generic character—a fact for which it is ‘almost superfluous to adduce evidence.’¹ This, again, is what we find in habit: take the case of a man who, from his youth up, has daily repeated a certain form of words. If in middle life an addition is made to the formula, he will find the recently acquired part more liable to vary than the rest.

Again, there is the wonderful fact that, as the ovum develops into the perfect organism, it passes through a series of changes which are believed to represent the successive forms through which its ancestors passed in the process of evolution. This is precisely paralleled by our own experience of memory, for it often happens that we cannot reproduce the last learned verse of a poem without repeating the earlier part; each verse is suggested by the previous one and acts as a stimulus for the next. The blurred and imperfect character of the ontogenetic version of the phylogenetic series may at least remind us of the tendency to abbreviate by omission what we have learned by heart.

In all bi-sexual organisms the ontogenetic rhythm of the offspring is a combination of the rhythms of its parents. This may or may not be visible in the offspring; thus in the crossing of two varieties the mongrel assumes the character of the prepotent parent. Or the offspring may show a blend of both parental characters. Semon² uses as a model the two versions of Goethe's poem—

· Ueber allen Gipfeln, ist Ruh, in allen { Wäldern, hörst du, keinen Hauch.
Wipfeln, spürest du, kaum einen Hauch.

One of these terminations will generally be prepotent, probably the one that was heard first or heard most often. But the cause of such prepotency may be as obscure as the corresponding occurrence in the formation of mongrels. We can only say that in some persons the word ‘allen’ releases the word ‘Wäldern,’ while in others it leads up to ‘Wipfeln.’ Again, a mixture of the terminations may occur leading to such a mongrel form as: ‘in allen Wäldern hörst du kaum einen Hauch.’ The same thing is true of music; a man with an imperfect memory easily interpolates in a melody a bar that belongs elsewhere. In the case of memory the introduction of a link from one mental rhythm into another can only occur when the two series are closely similar, and this may remind us of the difficulty of making a cross between distantly related forms.

Enough has been said to show that there is a resemblance between the two rhythms of development and of memory; and that there is at least a *prima facie* case for believing them to be essentially similar. It will be seen that my view is the same as that of Hering, which is generally described as the identification of memory and inheritance.³

¹ *Origin of Species*, 6th edit., p. 122.

² *Die Mneme*, 2nd edit., pp. 147, 221, 303, 345.

³ Everyone who deals with this subject must take his stand on the foundation laid by Hering in his celebrated address given at Vienna in 1870 and reprinted in No. 148 of Ostwald's *Erakt Klassiker*. The passage quoted (p. 14) is from Samuel

Hering says that 'between the *me* of to-day and the *me* of yesterday lie night and sleep, abysses of unconsciousness; nor is there any bridge but memory with which to span them.' And in the same way he claims that the abyss between two generations is bridged by the unconscious memory that resides in the germ cells. It is also the same as that of Semon and to a great extent as that of Rignano.¹ I, however, prefer at the moment to limit myself to asserting the identity of ontogeny and habit, or, more generally, to the assertion in Semon's phraseology, that ontogeny is a mnemonic phenomenon.

Evolution, in its modern sense, depends on a change in the ontogenetic rhythm. This is obvious, since if this rhythm is absolutely fixed, a species can never give rise to varieties. This being so, we have to ask *in what ways* the ontogenetic rhythm can be altered. An habitual action, for instance, a trick learned by a dog, may be altered by adding new accomplishments; at first the animal will persist in finishing his performance at the old place, but at last the extended trick will be bonded into a rhythm of actions as fixed as was the original simpler performance. May we not believe that this is what has occurred in evolution?

We know from experiment that a plant may be altered in form by causes acting on it during the progress of development. Thus a beech tree may be made to develop different forms of leaves by exposing it to sunshine or to shade. The ontogeny is different in the two cases, and what is of special interest is, that there exist shade-loving plants in which a structure similar to that of the shaded beech-leaf is apparently typical of the species, but on this point it is necessary to speak with caution. In the same way Goebel points out that in some orchids the assimilating roots take on a flattened form when exposed to sunlight, but in others this morphological change has become automatic, and occurs even in darkness.²

Such cases suggest at least the possibility of varieties arising as changes in or additions to the later stages of ontogeny. This is, briefly given, the epigenetic point of view.

But there is another way of looking at the matter—namely, that upheld by Galton and Weismann. According to this view ontogeny can only be changed by a fundamental upset of the whole system—namely, by an alteration occurring in its first stage, the germ cell, and this view is now very generally accepted.

The same type of change may conceivably occur in memory or habit, that is, the rhythm as a whole may be altered by some cause acting on the nerve-centres connected with the earlier links of the series. The

Butler's translation of Hering in *Unconscious Memory*, 1880, p. 110. Butler had previously elaborated the view that 'we are one person with our ancestors' in his entertaining book *Life and Habit*, 1878, and this was written in ignorance of Hering's views.

¹ *Sur la transmissibilité des caractères acquis*, Paris, 1906.

² Goebel's *Organography of Plants*, part ii. p. 285.

analogy is not exact, but such an imaginary case is at least of a different type from a change in habit consisting in the addition of a new link or the alteration of one of the latest formed links. If we were as ignorant of the growth of human actions as we are of variation, we might have a school of naturalists asserting that all changes in habit originate in the earliest link of the series. But we know that this is not the case. On the other hand, I fully admit that the structure of an ovum may in this way be altered, and give rise to a variation which may be the starting-point of a new species.

But how can a new species originate according to an epigenetic theory? How can a change in the latter stages of ontogeny produce a permanent alteration in the germ-cells? Our answer to this question will depend on our views of the structure of the germ-cells. According to the mnemic theory they have the quality which is found in the highest perfection in nerve-cells, but is at the same time a character of all living matter—namely, the power of retaining the residual effects of former stimuli and of giving forth or reproducing under certain conditions an echo of the original stimulus. In Semon's phraseology germ-cells must, like nerve-cells, contain engrams, and these engrams must be (like nerve-engrams) bonded together by association, so that they come into action one after another in a certain order automatically, *i.e.*, in the absence of the original stimuli.

This seems to me the strength of the mnemic theory—namely, that it accounts for the preformed character of germ-cells by the building up in them of an organised series of engrams. But if this view has its strength, it has also its weakness. Routine can only be built up by repetition, but each stage in ontogeny occurs only once in a lifetime. Therefore if ontogeny is a routine each generation must be mnemically connected with the next. This can only be possible if the germ-cells are, as it were, in telegraphic communication with the whole body of the organism; so that as ontogeny is changed by the addition of new characters, new engrams are added to the germ-cell.

Thus in fact the mnemic theory of development depends on the possibility of what is known as somatic inheritance or the inheritance of acquired characters. This is obvious to all those familiar with the subject, but to others it may not be so clear. Somatic inheritance is popularly interesting in relation to the possible inherited effects of education, or of mutilations, or of the effects of use and disuse. It is forgotten that it may be, as I have tried to show, an integral part of all evolutionary development.

Weismann's Theory.

Everyone must allow that if Weismann's theory of inheritance is accepted we cannot admit the possibility of somatic inheritance. This may be made clear to those unfamiliar with the subject by an illustration taken from the economy of an ant's nest or beehive. The queen¹ on

¹ Nor do the drones share the activity of the workers.

whom depends the future of the race is cut off from all active experience of life : she is a mere reproducing machine, housed, fed, and protected by the workers. But these, on whom falls the burden of the struggle for life and the experience of the world generally, are sterile, and take no direct share in the reproduction of the species. The queen represents Weismann's germ-plasm, the workers are the body or soma. Now imagine the colony exposed to some injurious change in environment ; the salvation of the species will depend on whether or no an improved pattern of worker can be produced. This depends on the occurrence of appropriate variations, so that the queen bee and the drones, on whom this depends, are of central importance. On the other hand any change occurring in the workers, for instance, increased skill due to practice in doing their work or changes in their structure due to external conditions, cannot possibly be inherited, since workers are absolutely cut off from the reproduction of the race. According to Weismann, there is precisely the same bar to the inheritance of somatic change.

The racial or phyletic life of all organisms is conceived by him as a series of germ-cells whose activity is limited to varying, and whose survival in any generation depends on the production of a successful soma or body capable of housing, protecting, and feeding the germ-cell. Most people would *a priori* declare that a community where experience and action are separated must fail. But the bee's nest, which must be allowed to be something more than an illustration of Weismann's theory, proves the contrary.

It is clear that there must be war to the knife between the theory of Weismann and that of the somatists—to coin a name for those who believe in the inheritance of acquired characters. A few illustrations may be given of the strength of Weismann's position. Some trick or trivial habit appears in two successive generations, and the son is said to inherit it from his father. But this is not necessarily a case of somatic inheritance, since according to Weismann the germ-plasm of both father and son contained the potentiality of the habit in question. If we keep constantly in view Weismann's theory of continuity, the facts which are supposed to prove somatic inheritance cease to be decisive.

Weismann has also shown by means of his hypothesis of 'simultaneous stimulation'¹ the unconvincingness of a certain type of experiment. Thus Fischer showed that when chrysalids of *Arctia caja* are subjected to low temperature a certain number of them produce dark-coloured insects ; and further that these moths mated together yield dark-coloured offspring. This has been held to prove somatic inheritance, but Weismann points out that it is explicable by the low temperature having an identical effect on the colour-determinants existing in the wing-rudiments of the pupa, and on the same determinants occurring in the germ-cells.

It does not seem to me worth while to go in detail into the evi-

¹ I borrow this convenient expression from Plate's excellent book, *Ueber die Bedeutung des Darwin'schen Selectionsprincipis*, 1903, p. 81.

dence by which somatists strive to prove their point, because I do not know of any facts which are really decisive. That is to say, that though they are explicable as due to somatic inheritance, they never seem to me absolutely inexplicable on Weismann's hypothesis. But, as already pointed out, it is not necessary to look for special facts and experiments, since if the mnemic theory of ontogeny is accepted the development of every organism in the world depends on somatic inheritance.

I fully acknowledge the strength of Weismann's position ; I acknowledge also most fully that it requires a stronger man than myself to meet that trained and well-trying fighter. Nevertheless, I shall venture on a few remarks. It must be remembered that, as Romanes¹ pointed out, Weismann has greatly strengthened his theory of heredity by giving up the absolute stability and perpetual continuity of germ-plasm. Germ-plasm is no longer that mysterious entity, immortal and self-contained, which used to suggest a physical soul. It is no longer the aristocrat it was when its only activity was dependent on its protozoan ancestors, when it reigned absolutely aloof from its contemporary subjects. The germ-plasm theory of to-day is liberalised, though it is not so democratic as its brother sovereign Pangenesis, who reigns, or used to reign, by an elaborate system of proportional representation. But in spite of the skill and energy devoted to its improvement by its distinguished author, Weismannism fails, in my opinion, to be a satisfactory theory of evolution.

All such theories must account for two things which are parts of a single process but may logically be considered separately : (i) The fact of ontogeny, namely, that the ovum has the capacity of developing into a certain more or less predetermined form ; (ii) The fact of heredity—the circumstance that this form is approximately the same as that of the parent.

The doctrine of pangenesis accounts for heredity, since the germ-cells are imagined as made up of gemmules representing all parts of the adult ; but it does not account for ontogeny, because there seems to me no sufficient reason why the gemmules should become active in a predetermined order unless, indeed, we allow that they do so by habit, and then the doctrine of pangenesis becomes a variant of the mnemic theory.

The strength of Weismann's theory lies in its explanation of heredity. According to the doctrine of continuity, a fragment of the germ-plasm is, as it were, put on one side and saved up to make the germ-cell of the new generation, so that the germ-cells of two successive generations are made of the same material. This again depends on Weismann's belief that when the ovum divides, the two daughter cells are not identical ; that in fact the fundamental difference between soma and germ-cells begins at this point. But this is precisely where many naturalists whose observations are worthy of all respect differ from him. Weismann's theory is therefore threatened at the very foundation.

¹ *An Examination of Weismann*, 1893, pp. 169, 170.

Even if we allow Weismann's method of providing for the identity between the germ-cell of two successive generations, there remains, as above indicated, a greater problem—namely, that of ontogeny. We no longer look at the potentiality of a germ-cell as Caliban looked on Setebos, as something essentially incomprehensible ruling the future in an unknown way—'just choosing so.' If the modern germ-cell is to have a poetic analogue it must be compared to a Pandora's box of architectonic sprites which are let loose in definite order, each serving as a master builder for a prescribed stage of ontogeny. Weismann's view of the mechanism by which his determinants—the architectonic sprites—come into action in due order is, I assume, satisfactory to many, but I confess that I find it difficult to grasp. The orderly distribution of determinants depends primarily on their arrangement in the *ids*, where they are held together by 'vital affinities.' They are guided to the cells on which they are to act by differential divisions, in each of which the determinants are sorted into two unequal lots. They then become active, *i.e.*, break up into biophores, partly under the influence of liberating stimuli and partly by an automatic process. Finally the biophores communicate a 'definite vital force' to the appropriate cells.¹ This *may* be a description of what happens; but inasmuch as it fails to connect the process of ontogeny with physiological processes of which we have definite knowledge, it does not to me seem a convincing explanation.

For myself I can only say that I am not satisfied with Weismann's theory of heredity or of ontogeny. As regards the first, I incline to deny the distinction between germ and soma, to insist on the plain facts that the soma is continuous with the germ-cell, and that the somatic cells may have the same reproductive qualities as the germ-cells (as is proved by the facts of regeneration); that, in fact, the germ-cell is merely a specialised somatic cell and has the essential qualities of the soma. With regard to ontogeny, I have already pointed out that Weismann does not seem to explain its automatic character.

The Mnemic Theory.

If the mnemic theory is compared with Weismann's views it is clear that it is strong precisely where these are weakest—namely, in giving a coherent theory of the rhythm of development. It also bears comparison with all theories in which the conception of determinants occurs. Why should we make elaborate theories of hypothetical determinants to account for the potentialities lying hidden in the germ-cell, and neglect the only determinants of whose existence we have positive knowledge (though we do not know their precise nature)? We know positively that by making a dog sit up and then giving him a biscuit we build up something in his brain in consequence of which a biscuit becomes the stimulus to the act of sitting. The mnemic theory assumes that the

¹ *The Evolution Theory*, Eng. trans., i. 373 *et seq.*

determinants of morphological change are of the same type as the structural alteration wrought in the dog's brain.

The mnemonic theory—at any rate that form of it held by Semon and by myself—agrees with the current view, viz., that the nucleus is the centre of development, or, in Semon's phraseology, that the nucleus contains the engrams in which lies the secret of the ontogenetic rhythm. But the mode of action of the mnemonic nucleus is completely different from that of Weismann. He assumes that the nucleus is disintegrated in the course of development by the dropping from it of the determinants which regulate the manner of growth of successive groups of cells. But if the potentiality of the germ nucleus depends on the presence of engrams, if, in fact, its function is comparable to that of a nerve-centre, its capacity is not diminished by action; it does not cast out engrams from its substance as Weismann's nucleus is assumed to drop armies of determinants. The engrams are but cut deeper into the records, and more closely bonded one with the next. The nucleus, considered as a machine, does not lose its component parts in the course of use. We shall see later on that the nuclei of the whole body may, on the mnemonic theory, be believed to become alike. The fact that the mnemonic theory allows the nucleus to retain its repeating or reproductive or mnemonic quality supplies the element of continuity. The germ-cell divides and its daughter cells form the tissues of the embryo, and in this process the original nucleus has given rise to a group of nuclei; these however have not lost their engrams, but retain the potentiality of the parent nucleus. We need not therefore postulate the special form of continuity which is characteristic of Weismann's theory.

We may say, therefore, that the mnemonic hypothesis harmonises with the facts of heredity and ontogeny. But the real difficulties remain to be considered, and these, I confess, are of a terrifying magnitude.

The first difficulty is the question how the changes arising in the soma are, so to speak, telegraphed to the germ-cells. Hering allows that such communication must at first seem highly mysterious.¹ He then proceeds to show how by the essential unity and yet extreme ramification of the nervous system 'all parts of the body are so connected that what happens in one echoes through the rest, so that from the disturbance occurring in any part some notification, faint though it may be, is conveyed to the most distant parts of the body.'

A similar explanation is given by Nägeli. He supposes that adaptive, in contradistinction to organic, characters are produced by external causes; and since these characters are hereditary there must be communication between the seat of adaptation and the germ-cells. This telegraphic effect is supposed to be effected by the network of idioplasm which traverses the body, in the case of plants by the intercellular protoplasmic threads.

¹ E. Hering in Ostwald's *Klassiker der exakten Wissenschaften*, No. 148, p. 14; see also S. Butler's translation in *Unconscious Memory*, p. 119.

Semon faces the difficulty boldly. When a new character appears in the body of an organism, in response to changing environment, Semon assumes that a new engram is added to the nuclei in the part affected ; and that, further, the disturbance tends to spread to all the nuclei of the body (including those of the germ-cells), and to produce in them the same change. In plants the flow must be conceived as travelling by intercellular plasmic threads, but in animals primarily by nerve-trunks. Thus the reproductive elements must be considered as having in some degree the character of nerve-cells. So that, for instance, if we are to believe that an individual habit may be inherited and appear as an instinct, the repetition of the habit will not merely mean changes in the central nervous system, but also corresponding changes in the germ-cells. These will be, according to Semon, excessively faint in comparison to the nerve-engrams, and can only be made efficient by prolonged action. Semon lays great stress on the slowness of the process of building up efficient engrams in the germ-cells.

Weismann¹ speaks of the impossibility of germinal engrams being formed in this way. He objects that nerve-currents can only differ from each other in intensity, and therefore there can be no communication of potentialities to the germ-cell. He holds it to be impossible that somatic changes should be telegraphed to the germ-cell and be reproduced ontogenetically—a process which he compares to a telegram despatched in German and arriving in Chinese. According to Semon² what radiates from the point of stimulation in the soma is the primary excitation set up in the somatic cells ; if this is so, the radiating influence will produce the same effect on all the nuclei of the organism. My own point of view is the following. In a plant (as already pointed out) the ectoplasm may be compared to the surface of the cell, and the primary excitation of the cell will be a change in the ectoplasm ; but since cells are connected by ectoplasmic threads the primary excitation will spread and produce in other cells a faint copy of the engram impressed on the somatic cells originally stimulated. But in all these assumptions we are met by the question to which Weismann has called attention—namely, whether nervous impulses can differ from one another in *quality* ?³ The general opinion of physiologists is undoubtedly to the opposite effect—namely, that all nervous impulses are identical in quality. But there are notable exceptions, for instance, Hering,⁴ who strongly

¹ Weismann, *The Evolution Theory*, 1904, vol. ii. p. 63 ; also his *Richard Semon's 'Mneme' und die Vererbung erworbener Eigenschaften*, in the *Archiv für Rassen- und Gesellschafts-Biologie*, 1906. Semon has replied in the same journal for 1907.

² Semon, *Mneme*, ed. i. p. 142, does not, however, consider it proved that the nucleus is necessarily the smallest element in which the whole inheritance resides. He refers especially to the regeneration of sections of *Stentor* which contain mere fragments of the nucleus.

³ I use this word in the ordinary sense without reference to what is known as *modality*.

⁴ *Zur Theorie der Nerventhätigkeit, Akademische Vortrag*, 1898 (Veit, Leipzig).

supports what may be called the qualitative theory. I am not competent to form an opinion on the subject, but I confess to being impressed by Hering's argument that the nerve-cell and nerve-fibre, as parts of one individual (the neuron), must have a common irritability. On the other hand there is striking evidence, in Langley's¹ experiments on the cross-grafting of efferent nerves, that here at least nerve impulses are interchangeable and therefore identical in quality. The state of knowledge as regards afferent nerves is, however, more favourable to my point of view. For the difficulties that meet the physiologist—especially as regards the nerves of smell and hearing—are so great that it has been found simpler to assume differences in impulse-quality, rather than attempt an explanation of the facts on the other hypothesis.²

On the whole it may be said that, although the trend of physiological opinion is against the general existence of qualitative differences in nerve-impulses, yet the question cannot be said to be settled either one way or the other.

Another obvious difficulty is to imagine how within a single cell the engrams or potentialities of a number of actions can be locked up. We can only answer that the nucleus is admittedly very complex in structure. It may be added (but this not an answer) that in this respect it claims no more than its neighbours; it need not be more complex than Weismann's germ-plasm. One conceivable simplification seems to be in the direction of the pangenes of De Vries. He imagines that these heritage-units are relatively small in number, and that they produce complex results by combination, not by each being responsible for a minute fraction of the total result.³ They may be compared to the letters of the alphabet which by combination make an infinity of words.⁴ Nägeli⁵ held a similar view. 'To understand heredity,' he wrote, 'we do not need a special independent symbol for every difference conditioned by space, time, or quality, but a substance which can represent every possible combination of differences by the fitting together of a limited number of elements, and which can be transformed by permutations into other combinations.' He applied (*loc. cit.*, p. 59) the idea of a combination of symbols to the telegraphic quality of his idioplasm. He suggests that as the nerves convey the most varied perceptions of external objects to the central nervous system, and there create a coherent picture, so it is not impossible that the idioplasm may convey a combination of its local alterations to other parts of the organism.

Another theory of simplified telegraphy between soma and germ-cell

¹ *Proc. R. Soc.*, 1904, p. 99. *Journal of Physiology*, xxiii. p. 240, and xxxi. p. 365.

² See Nagel, *Handbuch der Physiologie des Menschen*, iii. (1905), pp. 1-15.

³ De Vries, *Intracellular Pangenesis*, p. 7.

⁴ I take this comparison from Lotsy's account of De Vries' theory. Lotsy, *Vorlesungen über Deszendenztheorien*, 1906, i. p. 98.

⁵ Nägeli's *Abstammungslehre*, 1884, p. 73.

is given by Rignano.¹ I regret that the space at my command does not permit me to give a full account of his interesting speculation on somatic inheritance. It resembles the theories of Hering, Butler, and Semon in postulating a quality of living things, which is the basis both of memory and inheritance. But it differs from them in seeking for a physical explanation or model of what is common to the two. He compares the nucleus to an electric accumulator which in its discharge gives out the same sort of energy that it has received. How far this is an allowable parallel I am not prepared to say, and in what follows I have given Rignano's results in biological terms. What interests me is the conclusion that the impulse conveyed to the nucleus of the germ-cell is, as far as results are concerned, the external stimulus. Thus, if a somatic cell (A) is induced by an external stimulus (S) acting on the nucleus to assume a new manner of development, a disturbance spreads through the organism, so that finally the nuclei of the germ-cells are altered in a similar manner. When the cellular descendants of the germ-cells reach the same stage of ontogeny as that in which the original stimulation occurred, a stimulus comes into action equivalent to S as regards the results it is capable of producing. So that the change originally wrought in cell A by the actual stimulus S is now reproduced by what may be called an inherited stimulus. But when A was originally affected other cells, B, C, D, may have reacted to S by various forms of growth. And therefore when during the development of the altered germ-cell something equivalent to S comes into play, there will be induced, not merely the original change in the development of A, but also the changes which were originally induced in the growth of B, C, D. Thus, according to Rignano, the germ-nucleus releases a number of developmental processes, each of which would, according to Weismann, require a separate determinant.

If the view here given is accepted, we must take a new view of Weismann's cases of *simultaneous stimulation*, i.e., cases like Fischer's experiments on *Arctia caja*, which he does not allow to be somatic inheritance. If we are right in saying that, the original excitation of the soma is transferred to the germ-cell, and it does not matter whether the stimulus is transferred by 'telegraphy,' or whether a given cause, e.g., a low temperature, acts simultaneously on soma and germ-cell. In both cases we have a given alteration produced in the nuclei of the soma and the germ-cell. Nageli used the word *telegraphy* to mean a dynamic form of transference, but he did not exclude the possibility of the same effect being produced by the movement of chemical substances, and went so far as to suggest that the sieve tubes might convey such stimuli in plants. In any case this point of view² deserves careful consideration.

¹ For what is here given I am partly indebted to Signor Rignano's letters.

² See Semon, *Archiv f. Rassen- und Gesellschafts-Biologie*, 1907, p. 39.

Still another code of communication seems to me to be at least conceivable. One of the most obvious characteristics of animal life is the guidance of the organism by certain groups of stimuli, producing either a movement of seeking (positive reaction¹) or one of avoidance (negative reaction). Taking the latter as being the simplest, we find that in the lowest as in the highest organisms a given reaction follows each one of a number of diverse conditions which have nothing in common save that they are broadly harmful in character. We withdraw our hands from a heated body, a prick, a corrosive substance, or an electric shock. The interesting point is that it is left to the organism to discover by the method of trial and error the best means of dealing with a sub-injurious stimulus. May we not therefore say that the existence of pleasure and pain simplifies inheritance? It certainly renders unnecessary a great deal of detailed inheritance. The innumerable appropriate movements performed by animals are broadly the same as those of their parents, but they are not necessarily inherited in every detail; they are rather the unavoidable outcome of hereditary but unspecialised sensitiveness. It is as though heredity were arranged on a code-system instead of by separate signals for every movement of the organism.

It may be said that in individual life the penalty of failure is pain, but that the penalty for failure in ontogenetic morphology is death. But it is only because pain is the shadow cast by Death as he approaches that it is of value to the organism. Death would be still the penalty of creatures that had not acquired this sensitiveness to the edge of danger. Is it not possible that the sensitiveness to external agencies by which structural ontogeny is undoubtedly guided may have a similar quality, and that morphological variations may also be reactions to the edge of danger. But this is a point of view I cannot now enter upon.

It may be objected that the inheritance of anything so complex as an instinct is difficult to conceive on the mnemic theory. Yet it is impossible to avoid suspecting that at least some instincts originate in individual acquirements, since they are continuous with habits gained in the lifetime of the organism. Thus the tendency to peck at any small object is undoubtedly inherited; the power of distinguishing suitable from unsuitable objects is gained by experience. It may be said that the engrams concerned in the pecking instinct cannot conceivably be transferred from the central nervous system to the nucleus of the germ-cells. To this I might answer that this is not more inconceivable than Weismann's assumption that the germ-cell changes to be so altered that the young chicken pecks instinctively. Let us consider another case of what appears to be an hereditary movement. Take, for instance, the case of a young dog, who in fighting bites his own lips. The pain thus produced will induce him to tuck up his lips out of harm's way. This protective movement will become firmly associated with, not only the act of fighting,

¹ See Jennings, *Behavior of the Lower Organisms*.

but with the remembrance of it, and will show itself in the familiar snarl of the angry dog. This movement is now, I presume, hereditary in dogs, and is so strongly inherited by ourselves (from simian ancestors) that a lifting of the corner of the upper lip is a recognised signal of adverse feeling. Is it really conceivable that the original snarl is due to that unspecialised stimulus we call pain, whereas the inherited snarl is due to fortuitous upsets of the determinants in the germ-cell?

I am well aware that many other objections may be advanced against the views I advocate. To take a single instance, there are many cases where we should expect somatic inheritance, but where we look in vain for it. This difficulty, and others equally important, must for the present be passed over. Nor shall I say anything more as to the possible means of communication between the soma and the germ-cells. To me it seems conceivable that some such telegraphy is possible. But I shall hardly wonder if a majority of my hearers decide that the available evidence in its favour is both weak and fantastic. Nor can I wonder that, apart from the problem of mechanism, the existence of somatic inheritance is denied for want of evidence. But I must once more insist that, according to the mnemic hypothesis, somatic inheritance lies at the root of all evolution. Life is a gigantic experiment which the opposing schools interpret in opposite ways. I hope that in this dispute both sides will seek out and welcome decisive results. My own conviction in favour of somatic inheritance rests primarily on the automatic element in ontogeny. It seems to me certain that in development we have an actual instance of habit. If this is so, somatic inheritance must be a *vera causa*. Nor does it seem impossible that memory should rule the plasmic link which connects successive generations—the true miracle of the camel passing through the eye of a needle—since, as I have tried to show, the reactions of living things to their surroundings exhibit in the plainest way the universal presence of a mnemic factor.

We may fix our eyes on phylogeny and regard the living world as a great chain of forms, each of which has learned something of which its predecessors were ignorant; or we may attend rather to ontogeny, where the lessons learned become in part automatic. But we must remember that the distinction between phylogeny and ontogeny is an artificial one, and that routine and acquisition are blended in life.¹

The great engine of natural selection is taunted nowadays, as it was fifty years ago, with being merely a negative power. I venture to think that the mnemic hypothesis of evolution makes the positive value of natural selection more obvious. If evolution is a process of drilling organisms into habits, the elimination of those that cannot learn is an

¹ This subject is dealt with in a very interesting manner in Professor James Ward's forthcoming lectures on the *Realm of Ends*. Also in his article on *Mechanism and Morals* in the *Hibbert Journal*, October 1905, p. 92; and in his article on Psychology in the *Encyclopædia Britannica*, 1886, vol. xx: p. 44.

integral part of the process, and is no less real because it is carried out by a self-acting system. It is surely a positive gain to the harmony of the universe that the discordant strings should break. But natural selection does more than this ; and just as a trainer insists on his performing dogs accommodating themselves to conditions of increasing complexity, so does natural selection pass on its pupils from one set of conditions to other and more elaborate tests, insisting that they shall endlessly repeat what they have learned and forcing them to learn something new. Natural selection attains in a blind, mechanical way the ends gained by a human breeder ; and by an extension of the same metaphor it may be said to have the power of a trainer—of an automatic master with endless patience and all time at his disposal.

British Association for the Advancement of Science.

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ADDRESS

BY

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D.Sc., F.R.S.,
PRESIDENT.

TWENTY-FIVE years ago a great change was made in the practice of the British Association. From the foundation of our Society until 1884 its meetings had always been held in the British Isles; in that year, however, the Association met in Montreal, and a step was taken which changed us from an Insular into an Imperial Association. For this change, which now I think meets with nothing but approval, Canada is mainly responsible. Men of science welcome it for the increased opportunities it gives them of studying under the most pleasant and favourable conditions different parts of our Empire, of making new friends; such meetings as these not only promote the progress of science, but also help to strengthen the bonds which bind together the different portions of the King's Dominions.

This year, for the third time in a quarter of a century, we are meeting in Canada. As if to give us an object lesson in the growth of Empire, you in Winnipeg took the opportunity at our first meeting in Canada in 1884 to invite our members to visit Manitoba and see for themselves the development of the Province at that time. Those who were fortunate enough to be your guests then as well as now are confronted with a change which must seem to them unexampled and almost incredible. Great cities have sprung up, immense areas have

been converted from prairies to prosperous farms, flourishing industries have been started, and the population has quadrupled. As the President of a scientific association I hope I may be pardoned if I point out that even the enterprise and energy of your people and the richness of your country would have been powerless to effect this change without the resources placed at their disposal by the labours of men of science:

The eminence of my predecessors in the chair at the meetings of the British Association in Canada makes my task this evening a difficult one. The meeting at Montreal was presided over by Lord Rayleigh, who, like Lord Kelvin, his colleague in the chair of Section A at that meeting, has left the lion's mark on every department of physics, and who has shown that, vast as is the empire of physics, there are still men who can extend its frontiers in all of the many regions under its sway. It has been my lot to succeed Lord Rayleigh in other offices as well as this, and I know how difficult a man he is to follow.

The President of the second meeting in Canada—that held in 1897 at Toronto—was Sir John Evans, one of those men who, like Boyle, Cavendish, Darwin, and Huggins, have, from their own resources and without the aid derived from official positions or from the universities, made memorable contributions to science: such men form one of the characteristic features of British science. May we not hope that, as the knowledge of science and the interest taken in it increase, more of the large number of men of independent means in our country may be found working for the advancement of science, and thereby rendering services to the community no less valuable than the political, philanthropic, and social work at which many of them labour with so much zeal and success?

I can, however, claim to have some experience of, at any rate, one branch of Canadian science, for it has been my privilege to receive at the Cavendish Laboratory many students from your universities. Some of these have been holders of what are known as the 1851 scholarships. These scholarships are provided from the surplus of the Great Exhibition of 1851, and are placed at the disposal of most of the younger universities in the British Empire, to enable students to devote themselves for two or three years to original research in various branches of science. I have had many opportunities of seeing the work of these scholars, and I should like to put on record my opinion that there is no educational endowment in the country which has done or is doing better work.

I have had, as I said, the privilege of having as pupils students from your universities as well as from those of New Zealand, Australia, and the United States, and have thus had opportunities of comparing the effect on the best men of the educational system in force at your

universities with that which prevails in the older English universities. Well, as the result, I have come to the conclusion that there is a good deal in the latter system which you have been wise not to imitate. The chief evil from which we at Cambridge suffer and which you have avoided is, I am convinced, the excessive competition for scholarships which confronts our students at almost every stage of their education. You may form some estimate of the prevalence of these scholarships if I tell you that the colleges in the University of Cambridge alone give more than 35,000*l.* a year in scholarships to undergraduates, and I suppose the case is much the same at Oxford. The result of this is that preparation for these scholarships dominates the education of the great majority of the cleverer boys who come to these universities, and indeed in some quarters it seems to be held that the chief duty of a schoolmaster, and the best test of his efficiency, is to make his boys get scholarships. The preparation for the scholarship too often means that about two years before the examination the boy begins to specialise, and from the age of sixteen does little else than the subject, be it mathematics, classics, or natural science, for which he wishes to get a scholarship; then, on entering the university, he spends three or four years studying the same subject before he takes his degree, when his real life work ought to begin. How has this training fitted him for this work? I will take the case in which the system might perhaps be expected to show to greatest advantage, when his work is to be original research in the subject he has been studying. He has certainly acquired a very minute acquaintance with his subject—indeed, the knowledge possessed by some of the students trained under this system is quite remarkable, much greater than that of any other students I have ever met. But though he has acquired knowledge, the effect of studying one subject, and one subject only, for so long a time is too often to dull his enthusiasm for it, and he begins research with much of his early interest and keenness evaporated. Now there is hardly any quality more essential to success in research than enthusiasm. Research is difficult, laborious, often disheartening. The carefully designed apparatus refuses to work, it develops defects which may take months of patient work to rectify, the results obtained may appear inconsistent with each other and with every known law of Nature, sleepless nights and laborious days may seem only to make the confusion more confounded, and there is nothing for the student to do but to take for his motto ‘It’s dogged as does it,’ and plod on, comforting himself with the assurance that when success does come, the difficulties he has overcome will increase the pleasure—one of the most exquisite men can enjoy—of getting some conception which will make all that was tangled, confused, and contradictory clear and consistent. Unless he has enthusiasm to carry him on when the prospect seems almost

hopeless and the labour and strain incessant, the student may give up his task and take to easier, though less important, pursuits.

I am convinced that no greater evil can be done to a young man than to dull his enthusiasm. In a very considerable experience of students of physics beginning research, I have met with more—many more—failures from lack of enthusiasm and determination than from any lack of knowledge or of what is usually known as cleverness.

This continual harping from an early age on one subject, which is so efficient in quenching enthusiasm, is much encouraged by the practice of the colleges to give scholarships for proficiency in one subject alone. I went through a list of the scholarships awarded in the University of Cambridge last winter, and, though there were 202 of them, I could only find three cases in which it was specified that the award was made for proficiency in more than one subject.

The premature specialisation fostered by the preparation for these scholarships, injures the student by depriving him of adequate literary culture, while when it extends, as it often does, to specialisation in one or two branches of science, it retards the progress of science by tending to isolate one science from another. The boundaries between the sciences are arbitrary, and tend to disappear as science progresses. The principles of one science often find most striking and suggestive illustrations in the phenomena of another. Thus, for example, the physicist finds in astronomy that effects he has observed in the laboratory are illustrated on the grand scale in the sun and stars. No better illustration of this could be given than Professor Hale's recent discovery of the Zeeman effect in the light from sunspots; in chemistry, too, the physicist finds in the behaviour of whole series of reactions illustrations of the great laws of thermodynamics, while if he turns to the biological sciences he is confronted by problems, mostly unsolved, of unsurpassed interest. Consider for a moment the problem presented by almost any plant—the characteristic and often exquisite detail of flower, leaf, and habit—and remember that the mechanism which controls this almost infinite complexity was once contained in a seed perhaps hardly large enough to be visible. We have here one of the most entrancing problems in chemistry and physics it is possible to conceive.

Again the specialisation prevalent in schools often prevents students of science from acquiring sufficient knowledge of mathematics; it is true that most of those who study physics do some mathematics, but I hold that, in general, they do not do enough, and that they are not as efficient physicists as they would be if they had a wider knowledge of that subject. There seems at present a tendency in some quarters to discourage the use of mathematics in physics; indeed, one might infer, from the statements of some writers in quasi-scientific journals,

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that ignorance of mathematics is almost a virtue. If this is so, then surely of all the virtues this is the easiest and most prevalent.

I do not for a moment urge that the physicist should confine himself to looking at his problems from the mathematical point of view; on the contrary, I think a famous French mathematician and physicist was guilty of only slight exaggeration when he said that no discovery was really important or properly understood by its author unless and until he could explain it to the first man he met in the street.

But two points of view are better than one, and the physicist who is also a mathematician possesses a most powerful instrument for scientific research with which many of the greatest discoveries have been made; for example, electric waves were discovered by mathematics long before they were detected in the laboratory. He has also at his command a language clear, concise, and universal, and there is no better way of detecting ambiguities and discrepancies in his ideas than by trying to express them in this language. Again, it often happens that we are not able to appreciate the full significance of some physical discovery until we have subjected it to mathematical treatment, when we find that the effect we have discovered involves other effects which have not been detected, and we are able by this means to duplicate the discovery. Thus James Thomson, starting from the fact that ice floats on water, showed that it follows by mathematics that ice can be melted and water prevented from freezing by pressure. This effect, which was at that time unknown, was afterwards verified by his brother, Lord Kelvin. Multitudes of similar duplication of physical discoveries by mathematics could be quoted.

I have been pleading in the interests of physics for a greater study of mathematics by physicists. I would also plead for a greater study of physics by mathematicians in the interest of pure mathematics.

The history of pure mathematics shows that many of the most important branches of the subject have arisen from the attempts made to get a mathematical solution of a problem suggested by physics. Thus the differential calculus arose from attempts to deal with the problem of moving bodies. Fourier's theorem resulted from attempts to deal with the vibrations of strings and the conduction of heat; indeed, it would seem that the most fruitful crop of scientific ideas is produced by cross-fertilisation between the mind and some definite fact, and that the mind by itself is comparatively unproductive.

I think, if we could trace the origin of some of our most comprehensive and important scientific ideas, it would be found that they arose in the attempt to find an explanation of some apparently trivial and very special phenomenon; when once started the ideas grew to such generality and importance that their modest origin could hardly be suspected. Water vapour we know will refuse to condense into rain

unless there are particles of dust to form nuclei; so an idea before taking shape seems to require a nucleus of solid fact round which it can condense.

I have ventured to urge the closer union between mathematics and physics, because I think of late years there has been some tendency for these sciences to drift apart, and that the workers in applied mathematics are relatively fewer than they were some years ago. This is no doubt due to some extent to the remarkable developments made in the last few years in experimental physics on the one hand and in the most abstract and metaphysical parts of pure mathematics on the other. The fascination of these has drawn workers to the frontiers of these regions who would otherwise have worked nearer the junction of the two. In part, too, it may be due to the fact that the problems with which the applied mathematician has to deal are exceedingly difficult, and many may have felt that the problems presented by the older physics have been worked over so often by men of the highest genius that there was but little chance of any problem which they could have any hope of solving being left.

But the newer developments of physics have opened virgin ground which has not yet been worked over and which offers problems to the mathematician of great interest and novelty—problems which will suggest and require new methods of attack, the development of which will advance pure mathematics as well as physics.

I have alluded to the fact that pure mathematicians have been indebted to the study of concrete problems for the origination of some of their most valuable conceptions; but though no doubt pure mathematicians are in many ways very exceptional folk, yet in this respect they are very human. Most of us need to tackle some definite difficulty before our minds develop whatever powers they may possess. This is true for even the youngest of us, for our school boys and school girls, and I think the moral to be drawn from it is that we should aim at making the education in our schools as little bookish and as practical and concrete as possible.

I once had an illustration of the power of the concrete in stimulating the mind which made a very lasting impression upon me. One of my first pupils came to me with the assurance from his previous teacher that he knew little and cared less about mathematics, and that he had no chance of obtaining a degree in that subject. For some time I thought this estimate was correct, but he happened to be enthusiastic about billiards, and when we were reading that part of mechanics which deals with the collision of elastic bodies I pointed out that many of the effects he was constantly observing were illustrations of the subject we were studying. From that time he was a changed man. He had never before regarded mathematics as anything but a means of annoying

innocent undergraduates; now, when he saw what important results it could obtain, he became enthusiastic about it, developed very considerable mathematical ability, and, though he had already wasted two out of his three years at college, took a good place in the Mathematical Tripos.

It is possible to read books, to pass examinations without the higher qualities of the mind being called into play. Indeed, I doubt if there is any process in which the mind is more quiescent than in reading without interest. I might appeal to the widespread habit of reading in bed as a prevention of insomnia as a proof of this. But it is not possible for a boy to make a boat or for a girl to cook a dinner without using their brains. With practical things the difficulties have to be surmounted, the boat must be made watertight, the dinner must be cooked, while in reading there is always the hope that the difficulties which have been slurred over will not be set in the examination.

I think it was Helmholtz who said that often in the course of a research more thought and energy were spent in reducing a refractory piece of brass to order than in devising the method or planning the scheme of campaign. This constant need for thought and action gives to original research in any branch of experimental science great educational value even for those who will not become professional men of science. I have had considerable experience with students beginning research in experimental physics, and I have always been struck by the quite remarkable improvement in judgment, independence of thought and maturity produced by a year's research. Research develops qualities which are apt to atrophy when the student is preparing for examinations, and, quite apart from the addition of new knowledge to our store, is of the greatest importance as a means of education.

It is the practice in many universities to make special provision for the reception of students from other universities who wish to do original research or to study the more advanced parts of their subject, and considerable numbers of such students migrate from one university to another. I think it would be a good thing if this practice were to extend to students at an earlier stage in their career; especially should I like to see a considerable interchange of students between the universities in the Mother Country and those in the Colonies.

I am quite sure that many of our English students, especially those destined for public life, could have no more valuable experience than to spend a year in one or other of your universities, and I hope some of your students might profit by a visit to ours.

I can think of nothing more likely to lead to a better understanding of the feelings, the sympathies, and, what is not less important, the prejudices, of one country by another, than by the youths of those countries spending a part of their student life together.

Undergraduates as a rule do not wear a mask either of politeness or any other material, and have probably a better knowledge of each other's opinions and points of view—in fact, know each other better than do people of riper age. To bring this communion of students about there must be co-operation between the universities throughout the Empire; there must be recognition of each other's examinations, residence, and degrees. Before this can be accomplished there must, as my friend Mr. E. B. Sargant pointed out in a lecture given at the McGill University, be co-operation and recognition between the universities in each part of the Empire. I do not mean for a moment that all universities in a country should be under one government. I am a strong believer in the individuality of universities, but I do not think this is in any way inconsistent with the policy of an open door from one university to every other in the Empire.

It has usually been the practice of the President of this Association to give some account of the progress made in the last few years in the branch of science which he has the honour to represent.

I propose this evening to follow that precedent and to attempt to give a very short account of some of the more recent developments of physics, and the new conceptions of physical processes to which they have led.

The period which has elapsed since the Association last met in Canada has been one of almost unparalleled activity in many branches of physics, and many new and unsuspected properties of matter and electricity have been discovered. The history of this period affords a remarkable illustration of the effect which may be produced by a single discovery; for it is, I think, to the discovery of the Röntgen rays that we owe the rapidity of the progress which has recently been made in physics. A striking discovery like that of the Röntgen rays acts much like the discovery of gold in a sparsely populated country; it attracts workers who come in the first place for the gold, but who may find that the country has other products, other charms, perhaps even more valuable than the gold itself. The country in which the gold was discovered in the case of the Röntgen rays was the department of physics dealing with the discharge of electricity through gases, a subject which, almost from the beginning of electrical science, had attracted a few enthusiastic workers, who felt convinced that the key to unlock the secret of electricity was to be found in a vacuum tube. Röntgen, in 1895, showed that when electricity passed through such a tube, the tube emitted rays which could pass through bodies opaque to ordinary light; which could, for example, pass through the flesh of the body and throw a shadow of the bones on a suitable screen. The fascination of this discovery attracted many workers to the subject of the discharge of electricity through gases, and led to great improvements in the instruments used in this type of research. It is not, however, to

the power of probing dark places, important though this is, that the influence of Röntgen rays on the progress of science has mainly been due; it is rather because these rays make gases, and, indeed, solids and liquids, through which they pass conductors of electricity. It is true that before the discovery of these rays other methods of making gases conductors were known, but none of these was so convenient for the purposes of accurate measurement.

The study of gases exposed to Röntgen rays has revealed in such gases the presence of particles charged with electricity; some of these particles are charged with positive, others with negative electricity.

The properties of these particles have been investigated; we know the charge they carry, the speed with which they move under an electric force, the rate at which the oppositely charged ones recombine, and these investigations have thrown a new light, not only on electricity, but also on the structure of matter.

We know from these investigations that electricity, like matter, is molecular in structure, that just as a quantity of hydrogen is a collection of an immense number of small particles called molecules, so a charge of electricity is made up of a great number of small charges, each of a perfectly definite and known amount.

Helmholtz said in 1880 that in his opinion the evidence in favour of the molecular constitution of electricity was even stronger than that in favour of the molecular constitution of matter. How much stronger is that evidence now, when we have measured the charge on the unit and found it to be the same from whatever source the electricity is obtained. Nay, further, the molecular theory of matter is indebted to the molecular theory of electricity for the most accurate determination of its fundamental quantity, the number of molecules in any given quantity of an elementary substance.

The great advantage of the electrical methods for the study of the properties of matter is due to the fact that whenever a particle is electrified it is very easily identified, whereas an uncharged molecule is most elusive; and it is only when these are present in immense numbers that we are able to detect them. A very simple calculation will illustrate the difference in our power of detecting electrified and unelectrified molecules. The smallest quantity of unelectrified matter ever detected is probably that of neon, one of the inert gases of the atmosphere. Professor Strutt has shown that the amount of neon in $\frac{1}{30}$ of a cubic centimetre of the air at ordinary pressures can be detected by the spectroscope; Sir William Ramsay estimates that the neon in the air only amounts to one part of neon in 100,000 parts of air, so that the neon in $\frac{1}{30}$ of a cubic centimetre of air would only occupy at atmospheric pressure a volume of half a millionth of a cubic centimetre. When stated in this form the quantity seems exceedingly small, but in this small volume there are about

ten million million molecules. Now the population of the earth is estimated at about fifteen hundred millions, so that the smallest number of molecules of neon we can identify is about 7,000 times the population of the earth. In other words, if we had no better test for the existence of a man than we have for that of an unelectrified molecule we should come to the conclusion that the earth is uninhabited. Contrast this with our power of detecting electrified molecules. We can by the electrical method, even better by the cloud method of C. T. R. Wilson, detect the presence of three or four charged particles in a cubic centimetre. Rutherford has shown that we can detect the presence of a single α particle. Now the particle is a charged atom of helium; if this atom had been uncharged we should have required more than a million million of them, instead of one, before we should have been able to detect them.

We may, I think, conclude, since electrified particles can be studied with so much greater ease than unelectrified ones, that we shall obtain a knowledge of the ultimate structure of electricity before we arrive at a corresponding degree of certainty with regard to the structure of matter.

We have already made considerable progress in the task of discovering what the structure of electricity is. We have known for some time that of one kind of electricity—the negative—and a very interesting one it is. We know that negative electricity is made up of units all of which are of the same kind; that these units are exceedingly small compared with even the smallest atom, for the mass of the unit is only $\frac{1}{1700}$ part of the mass of an atom of hydrogen; that its radius is only 10^{-13} centimetre, and that these units, 'corpuscles' as they have been called, can be obtained from all substances. The size of these corpuscles is on an altogether different scale from that of atoms; the volume of a corpuscle bears to that of the atom about the same relation as that of a speck of dust to the volume of this room. Under suitable conditions they move at enormous speeds which approach in some instances the velocity of light.

The discovery of these corpuscles is an interesting example of the way Nature responds to the demands made upon her by mathematicians. Some years before the discovery of corpuscles it had been shown by a mathematical investigation that the mass of a body must be increased by a charge of electricity. This increase, however, is greater for small bodies than for large ones, and even bodies as small as atoms are hopelessly too large to show any appreciable effect; thus the result seemed entirely academic. After a time corpuscles were discovered, and these are so much smaller than the atom that the increase in mass due to the charge becomes not merely appreciable, but so great that, as the experiments of Kaufmann and Bucherer have shown, the whole of the mass of the corpuscle arises from its charge.

We know a great deal about negative electricity; what do we know about positive electricity? Is positive electricity molecular in structure? Is it made up into units, each unit carrying a charge equal in magnitude though opposite in sign to that carried by a corpuscle? Does, or does not, this unit differ, in size and physical properties, very widely from the corpuscle? We know that by suitable processes we can get corpuscles out of any kind of matter, and that the corpuscles will be the same from whatever source they may be derived. Is a similar thing true for positive electricity? Can we get, for example, a positive unit from oxygen of the same kind as that we get from hydrogen?

For my own part, I think the evidence is in favour of the view that we can, although the nature of the unit of positive electricity makes the proof much more difficult than for the negative unit.

In the first place we find that the positive particles—'canalstrahlen' is their technical name—discovered by our distinguished guest, Dr. Goldstein, which are found when an electric discharge passes through a highly rarefied gas, are, when the pressure is very low, the same, whatever may have been the gas in the vessel to begin with. If we pump out the gas until the pressure is too low to allow the discharge to pass, and then introduce a small quantity of gas and restart the discharge, the positive particles are the same whatever kind of gas may have been introduced.

I have, for example, put into the exhausted vessel oxygen, argon, helium, the vapour of carbon tetrachloride, none of which contain hydrogen, and found the positive particles ~~to be~~ the same as when hydrogen was introduced.

Some experiments made lately by Wellisch, in the Cavendish Laboratory, strongly support the view that there is a definite unit of positive electricity independent of the gas from which it is derived; these experiments were on the velocity with which positive particles move through mixed gases. If we have a mixture of methyl-iodide and hydrogen exposed to Röntgen rays, the effect of the rays on the methyl-iodide is so much greater than on the hydrogen that, even when the mixture contains only a small percentage of methyl-iodide, practically all the electricity comes from this gas, and not from the hydrogen.

Now if the positive particles were merely the residue left when a corpuscle had been abstracted from the methyl-iodide, these particles would have the dimensions of a molecule of methyl-iodide; this is very large and heavy, and would therefore move more slowly through the hydrogen molecules than the positive particles derived from hydrogen itself, which would, on this view, be of the size and weight of the light hydrogen molecules. Wellisch found that the velocities of both the positive and negative particles through the mixture were the same as the velocities through pure hydrogen, although in the one case the ions had originated from methyl-iodide and in the other from hydrogen; a

similar result was obtained when carbon tetrachloride, or mercury methyl, was used instead of methyl-iodide. These and similar results lead to the conclusion that the atom of the different chemical elements contain definite units of positive as well as of negative electricity, and that the positive electricity, like the negative, is molecular in structure.

The investigations made on the unit of positive electricity show that it is of quite a different kind from the unit of negative, the mass of the negative unit is exceedingly small compared with any atom, the only positive units that up to the present have been detected are quite comparable in mass with the mass of an atom of hydrogen; in fact they seem equal to it. This makes it more difficult to be certain that the unit of positive electricity has been isolated, for we have to be on our guard against its being a much smaller body attached to the hydrogen atoms which happen to be present in the vessel. If the positive units have a much greater mass than the negative ones, they ought not to be so easily deflected by magnetic forces when moving at equal speeds; and in general the insensibility of the positive particles to the influence of a magnet is very marked; though there are cases when the positive particles are much more readily deflected, and these have been interpreted as proving the existence of positive units comparable in mass with the negative ones. I have found, however, that in these cases the positive particles are moving very slowly, and that the ease with which they are deflected is due to the smallness of the velocity and not to that of the mass. It should, however, be noted that M. Jean Becquerel has observed in the absorption spectra of some minerals, and Professor Wood in the rotation of the plane of polarisation by sodium vapour, effects which could be explained by the presence in the substances of positive units comparable in mass with corpuscles. This, however, is not the only explanation which can be given of these effects, and at present the smallest positive electrified particles of which we have direct experimental evidence have masses comparable with that of an atom of hydrogen.

A knowledge of the mass and size of the two units of electricity, the positive and the negative, would give us the material for constructing what may be called a molecular theory of electricity, and would be a starting-point for a theory of the structure of matter; for the most natural view to take, as a provisional hypothesis, is that matter is just a collection of positive and negative units of electricity, and that the forces which hold atoms and molecules together, the properties which differentiate one kind of matter from another, all have their origin in the electrical forces exerted by positive and negative units of electricity, grouped together in different ways in the atoms of the different elements.

As it would seem that the units of positive and negative electricity

are of very different sizes, we must regard matter as a mixture containing systems of very different types, one type corresponding to the small corpuscle, the other to the large positive unit.

Since the energy associated with a given charge is greater the smaller the body on which the charge is concentrated, the energy stored up in the negative corpuscles will be far greater than that stored up by the positive. The amount of energy which is stored up in ordinary matter in the form of the electrostatic potential energy of its corpuscles is, I think, not generally realised. All substances give out corpuscles, so that we may assume that each atom of a substance contains at least one corpuscle. From the size and the charge on the corpuscle, both of which are known, we find that each corpuscle has 8×10^{-7} ergs of energy; this is on the supposition that the usual expressions for the energy of a charged body hold when, as in the case of a corpuscle, the charge is reduced to one unit. Now in one gramme of hydrogen there are about 6×10^{23} atoms, so if there is only one corpuscle in each atom the energy due to the corpuscles in a gramme of hydrogen would be 48×10^{16} ergs, or 11×10^9 calories. This is more than seven times the heat developed by one gramme of radium, or than that developed by the burning of five tons of coal. Thus we see that even ordinary matter contains enormous stores of energy; this energy is fortunately kept fast bound by the corpuscles; if at any time an appreciable fraction were to get free the earth would explode and become a gaseous nebula.

The matter of which I have been speaking so far is the material which builds up the earth, the sun, and the stars, the matter studied by the chemist, and which he can represent by a formula; this matter occupies, however, but an insignificant fraction of the universe, it forms but minute islands in the great ocean of the ether, the substance with which the whole universe is filled.

The ether is not a fantastic creation of the speculative philosopher; it is as essential to us as the air we breathe. For we must remember that we on this earth are not living on our own resources; we are dependent from minute to minute upon what we are getting from the sun, and the gifts of the sun are conveyed to us by the ether. It is to the sun that we owe not merely night and day, springtime and harvest, but it is the energy of the sun, stored up in coal, in waterfalls, in food, that practically does all the work of the world.

How great is the supply the sun lavishes upon us becomes clear when we consider that the heat received by the earth under a high sun and a clear sky is equivalent, according to the measurements of Langley, to about 7,000 horse-power per acre. Though our engineers have not yet discovered how to utilise this enormous supply of power, they will, I have not the slightest doubt, ultimately succeed in doing so;

and when coal is exhausted and our water-power inadequate, it may be that this is the source from which we shall derive the energy necessary for the world's work. When that comes about, our centres of industrial activity may perhaps be transferred to the burning deserts of the Sahara, and the value of land determined by its suitability for the reception of traps to catch sunbeams.

This energy, in the interval between its departure from the sun and its arrival at the earth, must be in the space between them. Thus this space must contain something which, like ordinary matter, can store up energy, which can carry at an enormous pace the energy associated with light and heat, and which can, in addition, exert the enormous stresses necessary to keep the earth circling round the sun and the moon round the earth.

The study of this all-pervading substance is perhaps the most fascinating and important duty of the physicist.

On the electromagnetic theory of light, now universally accepted, the energy streaming to the earth travels through the ether in electric waves; thus practically the whole of the energy at our disposal has at one time or another been electrical energy. The ether must, then, be the seat of electrical and magnetic forces. We know, thanks to the genius of Clerk Maxwell, the founder and inspirer of modern electrical theory, the equations which express the relation between these forces, and although for some purposes these are all we require, yet they do not tell us very much about the nature of the ether.

The interest inspired by equations, too, in some minds is apt to be somewhat Platonic; and something more grossly mechanical—a model, for example, is felt by many to be more suggestive and manageable, and for them a more powerful instrument of research, than a purely analytical theory.

Is the ether dense or rare? Has it a structure? Is it at rest or in motion? are some of the questions which force themselves upon us.

Let us consider some of the facts known about the ether. When light falls on a body and is absorbed by it, the body is pushed forward in the direction in which the light is travelling, and if the body is free to move it is set in motion by the light. Now it is a fundamental principle of dynamics that when a body is set moving in a certain direction, or, to use the language of dynamics, acquires momentum in that direction, some other mass must lose the same amount of momentum; in other words, the amount of momentum in the universe is constant. Thus when the body is pushed forward by the light some other system must have lost the momentum the body acquires, and the only other system available is the wave of light falling on the body; hence we conclude that there must have been momentum in the wave in the direction in which it is travelling. Momentum, however, implies mass in motion. We con-

clude, then, that in the ether through which the wave is moving there is mass moving with the velocity of light. The experiments made on the pressure due to light enable us to calculate this mass, and we find that in a cubic kilometre of ether carrying light as intense as sunlight is at the surface of the earth, the mass moving is only about one-fifty-millionth of a milligramme. We must be careful not to confuse this with the mass of a cubic kilometre of ether; it is only the mass moved when the light passes through it; the vast majority of the ether is left undisturbed by the light. Now, on the electro-magnetic theory of light, a wave of light may be regarded as made up of groups of lines of electric force moving with the velocity of light; and if we take this point of view we can prove that the mass of ether per cubic centimetre carried along is proportional to the energy possessed by these lines of electric force per cubic centimetre, divided by the square of the velocity of light. But though lines of electric force carry some of the ether along with them as they move, the amount so carried, even in the strongest electric fields we can produce, is but a minute fraction of the ether in their neighbourhood.

This is proved by an experiment made by Sir Oliver Lodge in which light was made to travel through an electric field in rapid motion. If the electric field had carried the whole of the ether with it, the velocity of the light would have been increased by the velocity of the electric field. As a matter of fact no increase whatever could be detected, though it would have been registered if it had amounted to one-thousandth part of that of the field.

The ether carried along by a wave of light must be an exceedingly small part of the volume through which the wave is spread. Parts of this volume are in motion, but by far the greater part is at rest; thus in the wave front there cannot be uniformity, at some parts the ether is moving, at others it is at rest—in other words, the wave front must be more analogous to bright specks on a dark ground than to a uniformly illuminated surface.

The place where the density of the ether carried along by an electric field rises to its highest value is close to a corpuscle, for round the corpuscles are by far the strongest electric fields of which we have any knowledge. We know the mass of the corpuscle, we know from Kaufmann's experiments that this arises entirely from the electric charge, and is therefore due to the ether carried along with the corpuscle by the lines of force attached to it.

A simple calculation shows that one-half of this mass is contained in a volume seven times that of a corpuscle. Since we know the volume of the corpuscle as well as the mass, we can calculate the density of the ether attached to the corpuscle; doing so, we find it amounts to the prodigious value of about 5×10^{10} , or about 2,000 million times that

of lead. Sir Oliver Lodge, by somewhat different considerations, has arrived at a value of the same order of magnitude.

Thus around the corpuscle ether must have an extravagant density: whether the density is as great as this in other places depends upon whether the ether is compressible or not. If it is compressible, then it may be condensed round the corpuscles, and there have an abnormally great density; if it is not compressible, then the density in free space cannot be less than the number I have just mentioned.

With respect to this point we must remember that the forces acting on the ether close to the corpuscle are prodigious. If the ether were, for example, an ideal gas whose density increased in proportion to the pressure, however great the pressure might be, then if, when exposed to the pressures which exist in some directions close to the corpuscle, it had the density stated above, its density under atmospheric pressure would only be about 8×10^{-6} , or a cubic kilometre would have a mass less than a gramme; so that instead of being almost incomparably denser than lead, it would be almost incomparably rarer than the lightest gas.

I do not know at present of any effect which would enable us to determine whether ether is compressible or not. And although at first sight the idea that we are immersed in a medium almost infinitely denser than lead might seem inconceivable, it is not so if we remember that in all probability matter is composed mainly of holes. We may, in fact, regard matter as possessing a bird-cage kind of structure in which the volume of the ether disturbed by the wires when the structure is moved is infinitesimal in comparison with the volume enclosed by them. If we do this, no difficulty arises from the great density of the ether; all we have to do is to increase the distance between the wires in proportion as we increase the density of the ether.

Let us now consider how much ether is carried along by ordinary matter, and what effects this might be expected to produce.

The simplest electrical system we know, an electrified sphere, has attached to it a mass of ether proportional to its potential energy, and such that if the mass were to move with the velocity of light its kinetic energy would equal the electrostatic potential energy of the particle. This result can be extended to any electrified system, and it can be shown that such a system binds a mass of the ether proportional to its potential energy. Thus a part of the mass of any system is proportional to the potential energy of the system.

The question now arises, Does this part of the mass add anything to the weight of the body? If the ether were not subject to gravitational attraction it certainly would not; and even if the ether were ponderable, we might expect that as the mass is swimming in a sea of ether it would not increase the weight of the body to which it is attached. But if it does not, then a body with a large amount of potential energy may have an

appreciable amount of its mass in a form which does not increase its weight, and thus the weight of a given mass of it may be less than that of an equal mass of some substance with a smaller amount of potential energy. Thus the weights of equal masses of these substances would be different. Now, experiments with pendulums, as Newton pointed out, enable us to determine with great accuracy the weights of equal masses of different substances. Newton himself made experiments of this kind, and found that the weights of equal masses were the same for all the materials he tried. Bessel, in 1830, made some experiments on this subject which are still the most accurate we possess, and he showed that the weights of equal masses of lead, silver, iron, brass did not differ by as much as one part in 60,000.

The substances tried by Newton and Bessel did not, however, include any of those substances which possess the marvellous power of radio-activity; the discovery of these came much later, and is one of the most striking achievements of modern physics.

These radio-active substances are constantly giving out large quantities of heat, presumably at the expense of their potential energy; thus when these substances reach their final non-radio-active state their potential energy must be less than when they were radio-active. Professor Rutherford's measurements show that the energy emitted by one gramme of radium in the course of its degradation to non-radio-active forms is equal to the kinetic energy of a mass of 1-13th of a milligramme moving with the velocity of light.

This energy, according to the rule I have stated, corresponds to a mass of 1-13th of a milligramme of the ether, and thus a gramme of radium in its radio-active state must have at least 1-13th of a milligramme more of ether attached to it than when it has been degraded into the non-radio-active forms. Thus if this ether does not increase the weight of the radium, the ratio of mass to weight for radium would be greater by about one part in 13,000 than for its non-radio-active products.

I attempted several years ago to find the ratio of mass to weight for radium by swinging a little pendulum, the bob of which was made of radium. I had only a small quantity of radium, and was not, therefore, able to attain any great accuracy. I found that the difference, if any, in the ratio of the mass to weight between radium and other substances was not more than one-part in 2,000. Lately we have been using at the Cavendish Laboratory a pendulum whose bob was filled with uranium oxide. We have got good reasons for supposing that uranium is a parent of radium, so that the great potential energy and large ethereal mass possessed by the radium will be also in the uranium; the experiments are not yet completed. It is, perhaps, expecting almost too much to hope that the radio-active substances may add to the great services they have

already done to science by furnishing the first case in which there is some differentiation in the action of gravity.

The mass of ether bound by any system is such that if it were to move with the velocity of light its kinetic energy would be equal to the potential energy of the system. This result suggests a new view of the nature of potential energy. Potential energy is usually regarded as essentially different from kinetic energy. Potential energy depends on the configuration of the system, and can be calculated from it when we have the requisite data; kinetic energy, on the other hand, depends upon the velocity of the system. According to the principle of the conservation of energy the one form can be converted into the other at a fixed rate of exchange, so that when one unit of one kind disappears a unit of the other simultaneously appears.

Now in many cases this rule is all that we require to calculate the behaviour of the system, and the conception of potential energy is of the utmost value in making the knowledge derived from experiment and observation available for mathematical calculation. It must, however, I think, be admitted that from the purely philosophical point of view it is open to serious objection. It violates, for example, the principle of continuity. When a thing changes from a state A to a different state B, the principle of continuity requires that it must pass through a number of states intermediate between A and B, so that the transition is made gradually, and not abruptly. Now, when kinetic energy changes into potential, although there is no discontinuity in the quantity of the energy, there is in its quality, for we do not recognise any kind of energy intermediate between that due to the motion and that due to the position of the system, and some portions of energy are supposed to change *per saltum* from the kinetic to the potential form. In the case of the transition of kinetic energy into heat energy in a gas, the discontinuity has disappeared with a fuller knowledge of what the heat energy in a gas is due to. When we were ignorant of the nature of this energy, the transition from kinetic into thermal energy seemed discontinuous; but now we know that this energy is the kinetic energy of the molecules of which the gas is composed, so that there is no change in the type of energy when the kinetic energy of visible motion is transformed into the thermal energy of a gas—it is just the transference of kinetic energy from one body to another.

If we regard potential energy as the kinetic energy of portions of the ether attached to the system, then all energy is kinetic energy, due to the motion of matter or of portions of ether attached to the matter. I showed, many years ago, in my 'Applications of Dynamics to Physics and Chemistry,' that we could imitate the effects of the potential energy of a system by means of the kinetic energy of invisible systems connected in an appropriate manner with the main system, and that

the potential energy of the visible universe may in reality be the kinetic energy of an invisible one connected up with it. We naturally suppose that this invisible universe is the luminiferous ether, that portions of the ether in rapid motion are connected with the visible systems, and that their kinetic energy is the potential energy of the systems.

We may thus regard the ether as a bank in which we may deposit energy and withdraw it at our convenience. The mass of the ether attached to the system will change as the potential energy changes, and thus the mass of a system whose potential energy is changing cannot be constant; the fluctuations in mass under ordinary conditions are, however, so small that they cannot be detected by any means at present at our disposal. Inasmuch as the various forms of potential energy are continually being changed into heat energy, which is the kinetic energy of the molecules of matter, there is a constant tendency for the mass of a system such as the earth or the sun to diminish, and thus as time goes on for the mass of ether gripped by the material universe to become smaller and smaller; the rate at which it would diminish would, however, get slower as time went on, and there is no reason to think that it would ever get below a very large value.

Radiation of light and heat from an incandescent body like the sun involves a constant loss of mass by the body. Each unit of energy radiated carries off its quota of mass, but as the mass ejected from the sun per year is only one part in 20 billionths ($1 \text{ in } 2 \times 10^{13}$) of the mass of the sun, and as this diminution in mass is not necessarily accompanied by any decrease in its gravitational attraction, we cannot expect to be able to get any evidence of this effect.

As our knowledge of the properties of light has progressed, we have been driven to recognise that the ether, when transmitting light, possesses properties which, before the introduction of the electromagnetic theory, would have been thought to be peculiar to an emission theory of light and to be fatal to the theory that light consists of undulations.

Take, for example, the pressure exerted by light. This would follow as a matter of course if we supposed light to be small particles moving with great velocities, for these, if they struck against a body, would manifestly tend to push it forward, while on the undulatory theory there seemed no reason why any effect of this kind should take place.

Indeed, in 1792, this very point was regarded as a test between the theories, and Bennet made experiments to see whether or not he could find any traces of this pressure. We now know that the pressure is there, and if Bennet's instrument had been more sensitive he must have observed it. It is perhaps fortunate that Bennet had not at his command more delicate apparatus. Had he discovered the pressure of light, it would have shaken confidence in the undulatory theory and

checked that magnificent work at the beginning of the last century which so greatly increased our knowledge of optics.

As another example, take the question of the distribution of energy in a wave of light. On the emission theory the energy in the light is the kinetic energy of the light particles. Thus the energy of light is made up of distinct units, the unit being the energy of one of the particles.

The idea that the energy has a structure of this kind has lately received a good deal of support. Planck, in a very remarkable series of investigations on the Thermodynamics of Radiation, pointed out that the expressions for the energy and entropy of radiant energy were of such a form as to suggest that the energy of radiation, like that of a gas on the molecular theory, was made up of distinct units, the magnitude of the unit depending on the colour of the light; and on this assumption he was able to calculate the value of the unit, and from this deduce incidentally the value of Avogadro's constant—the number of molecules in a cubic centimetre of gas at standard temperature and pressure.

This result is most interesting and important because if it were a legitimate deduction from the Second Law of Thermodynamics, it would appear that only a particular type of mechanism for the vibrators which give out light and the absorbers which absorb it could be in accordance with that law.

If this were so, then, regarding the universe as a collection of machines all obeying the laws of dynamics, the Second Law of Thermodynamics would only be true for a particular kind of machine.

There seems, however, grave objection to this view, which I may illustrate by the case of the First Law of Thermodynamics, the principle of the Conservation of Energy. This must be true whatever be the nature of the machines which make up the universe, provided they obey the laws of dynamics, any application of the principle of the Conservation of Energy could not discriminate between one type of machine and another.

Now, the Second Law of Thermodynamics, though not a dynamical principle in as strict a sense as the law of the Conservation of Energy, is one that we should expect to hold for a collection of a large number of machines of any type, provided that we could not directly affect the individual machines, but could only observe the average effects produced by an enormous number of them. On this view, the Second Law, as well as the First, should be incapable of saying that the machines were of any particular type: so that investigations founded on thermodynamics, though the expressions they lead to may suggest—cannot, I think, be regarded as proving—the unit structure of light energy.

It would seem as if in the application of thermodynamics to radiation some additional assumption has been implicitly introduced, for these applications lead to definite relations between the energy of the light of any particular wave length and the temperature of the luminous body.

Now a possible way of accounting for the light emitted by hot bodies is to suppose that it arises from the collisions of corpuscles with the molecules of the hot body, but it is only for one particular law of force between the corpuscles and the molecules that the distribution of energy would be the same as that deduced by the Second Law of Thermodynamics, so that in this case, as in the other, the results obtained by the application of thermodynamics to radiation would require us to suppose that the Second Law of Thermodynamics is only true for radiation when the radiation is produced by mechanism of a special type.

Quite apart, however, from considerations of thermodynamics, we should expect that the light from a luminous source should in many cases consist of parcels, possessing, at any rate to begin with, a definite amount of energy. Consider, for example, the case of a gas like sodium vapour, emitting light of a definite wave length; we may imagine that this light, consisting of electrical waves, is emitted by systems resembling Leyden jars. The energy originally possessed by such a system will be the electrostatic energy of the charged jar. When the vibrations are started, this energy will be radiated away into space, the radiation forming a complex system, containing, if the jar has no electrical resistance, the energy stored up in the jar.

The amount of this energy will depend on the size of the jar and the quantity of electricity with which it is charged. With regard to the charge, we must remember that we are dealing with systems formed out of single molecules, so that the charge will only consist of one or two natural units of electricity, or, at all events, some small multiple of that unit, while for geometrically similar Leyden jars the energy for a given charge will be proportional to the frequency of the vibration; thus, the energy in the bundle of radiation will be proportional to the frequency of the vibration.

We may picture to ourselves the radiation as consisting of the lines of electric force which, before the vibrations were started, were held bound by the charges on the jar, and which, when the vibrations begin, are thrown into rhythmic undulations, liberated from the jar and travel through space with the velocity of light.

Now let us suppose that this system strikes against an uncharged condenser and gives it a charge of electricity, the charge on the plates of the condenser must be at least one unit of electricity, because fractions of this charge do not exist, and each unit charge will anchor a unit tube of force, which must come from the parcel of radiation falling upon it. Thus a tube in the incident light will be anchored by the condenser, and the parcel formed by this tube will be anchored and withdrawn as a whole from the pencil of light incident on the condenser. If the energy required to charge up the condenser with a unit of electricity is greater than the energy in the incident parcel, the

tube will not be anchored and the light will pass over the condenser and escape from it. These principles that radiation is made up of units, and that it requires a unit possessing a definite amount of energy to excite radiation in a body on which it falls, perhaps receive their best illustration in the remarkable laws governing Secondary Röntgen radiation, recently discovered by Professor Barkla. Professor Barkla has found that each of the different chemical elements, when exposed to Röntgen rays, emit a definite type of secondary radiation whatever may have been the type of primary, thus lead emits one type, copper another, and so on; but these radiations are not excited at all if the primary radiation is of a softer type than the specific radiation emitted by the substance; thus the secondary radiation from lead being harder than that from copper; if copper is exposed to the secondary radiation from lead the copper will radiate, but lead will not radiate when exposed to copper. Thus, if we suppose that the energy in a unit of hard Röntgen rays is greater than that in one of soft, Barkla's results are strikingly analogous to those which would follow on the unit theory of light.

Though we have, I think, strong reasons for thinking that the energy in the light waves of definite wave length is done up into bundles, and that these bundles, when emitted, all possess the same amount of energy, I do not think there is any reason for supposing that in any casual specimen of light of this wave length, which may subsequent to its emission have been many times refracted or reflected, the bundles possess any definite amount of energy. For consider what must happen when a bundle is incident on a surface such as glass, when part of it is reflected and part transmitted. The bundle is divided into two portions, in each of which the energy is less than the incident bundle, and since these portions diverge and may ultimately be many thousands of miles apart, it would seem meaningless to still regard them as forming one unit. Thus the energy in the bundles of light, after they have suffered partial reflection, will not be the same as in the bundles when they were emitted. The study of the dimensions of these bundles, for example, the angle they subtend at the luminous source, is an interesting subject for investigation; experiments on interference between rays of light emerging in different directions from the luminous source would probably throw light on this point.

I now pass to a very brief consideration of one of the most important and interesting advances ever made in physics, and in which Canada, as the place of the labours of Professors Rutherford and Soddy, has taken a conspicuous part. I mean the discovery and investigation of radio-activity. Radio-activity was brought to light by the Röntgen rays. One of the many remarkable properties of these rays is to excite phosphorescence in certain substances, including the salts of uranium, when they fall upon them. Since Röntgen rays produce phosphor-

escence, it occurred to Becquerel to try whether phosphorescence would produce Röntgen rays. He took some uranium salts which had been made to phosphoresce by exposure, not to Röntgen rays but to sunlight, tested them, and found that they gave out rays possessing properties similar to Röntgen rays. Further investigation showed, however, that to get these rays it was not necessary to make the uranium phosphoresce, that the salts were just as active if they had been kept in the dark. It thus appeared that the property was due to the metal and not to the phosphorescence, and that uranium and its compounds possessed the power of giving out rays which, like Röntgen rays, affect a photographic plate, make certain minerals phosphoresce, and make gases through which they pass conductors of electricity.

Niepce de Saint-Victor had observed some years before this discovery that paper soaked in a solution of uranium nitrate affected a photographic plate, but the observation excited but little interest. The ground had not then been prepared, by the discovery of the Röntgen rays, for its reception, and it withered and was soon forgotten.

Shortly after Becquerel's discovery of uranium, Schmidt found that thorium possessed similar properties. Then Monsieur and Madame Curie, after a most difficult and laborious investigation, discovered two new substances, radium and polonium, possessing this property to an enormously greater extent than either thorium or uranium, and this was followed by the discovery of actinium by Debiere. Now the researches of Rutherford and others have led to the discovery of so many new radio-active substances that any attempts at christening seems to have been abandoned, and they are denoted, like policemen, by the letters of the alphabet.

Mr. Campbell has recently found that potassium, though far inferior in this respect to any of the substances I have named, emits an appreciable amount of radiation, the amount depending only on the quantity of potassium, and being the same whatever the source from which the potassium is obtained or whatever the elements with which it may be in combination.

The radiation emitted by these substances is of three types known as α , β and γ , rays. The α rays have been shown by Rutherford to be positively electrified atoms of helium, moving with speeds which reach up to about one-tenth of the velocity of light. The β rays are negatively electrified corpuscles, moving in some cases with very nearly the velocity of light itself, while the γ rays are unelectrified, and are analogous to the Röntgen rays.

The radio-activity of uranium was shown by Crookes to arise from something mixed with the uranium, and which differed sufficiently in properties from the uranium itself to enable it to be separated by chemical analysis. He took some uranium, and by chemical treat-

ment separated it into two portions, one of which was radio-active and the other not.

Next Becquerel found that if these two portions were kept for several months, the part which was not radio-active to begin with regained radio-activity, while the part which was radio-active to begin with had lost its radio-activity. These effects and many others receive a complete explanation by the theory of radio-active change which we owe to Rutherford and Soddy.

According to this theory, the radio-active elements are not permanent, but are gradually breaking up into elements of lower atomic weight; uranium, for example, is slowly breaking up, one of the products being radium, while radium breaks up into a radio-active gas called radium emanation, the emanation into another radio-active substance, and so on, and that the radiations are a kind of swan's song emitted by the atoms when they pass from one form to another; that, for example, it is when a radium atom breaks up and an atom of the emanation appears that the rays which constitute the radio-activity are produced.

Thus, on this view the atoms of the radio-active elements are not immortal, they perish after a life whose average value ranges from thousands of millions of years in the case of uranium to a second or so in the case of the gaseous emanation from actinium.

When the atoms pass from one state to another they give out large stores of energy, thus their descendants do not inherit the whole of their wealth of stored-up energy, the estate becomes less and less wealthy with each generation; we find, in fact, that the politician, when he imposes death duties, is but imitating a process which has been going on for ages in the case of these radio-active substances.

Many points of interest arise when we consider the rate at which the atoms of radio-active substance disappear. Rutherford has shown that whatever be the age of these atoms, the percentage of atoms which disappear in one second is always the same; another way of putting it is that the expectation of life of an atom is independent of its age—that an atom of radium one thousand years old is just as likely to live for another thousand years as one just sprung into existence.

Now this would be the case if the death of the atom were due to something from outside which struck old and young indiscriminately; in a battle, for example, the chance of being shot is the same for old and young; so that we are inclined at first to look to something coming from outside as the cause why an atom of radium, for example, suddenly changes into an atom of the emanation. But here we are met with the difficulty that no changes in the external conditions that we have as yet been able to produce have had any effect on the life of the atom; as far as we know at present the life of a radium atom is the same at the temperature of a furnace as at that of liquid air—it is not altered by sur-

rounding the radium by thick screens of lead or other dense materials to ward off radiation from outside, and what to my mind is especially significant, it is the same when the radium is in the most concentrated form, when its atoms are exposed to the vigorous bombardment from the rays given off by the neighbouring atoms, as when it is in the most dilute solution, when the rays are absorbed by the water which separates one atom from another. This last result seems to me to make it somewhat improbable that we shall be able to split up the atoms of the non-radio-active elements by exposing them to the radiation from radium; if this radiation is unable to affect the unstable radio-active atoms, it is somewhat unlikely that it will be able to affect the much more stable non-radio-active elements.

The evidence we have at present is against a disturbance coming from outside breaking up of the radio-active atoms, and we must therefore look to some process of decay in the atom itself; but if this is the case, how are we to reconcile it with the fact that the expectation of life of an atom does not diminish as the atom gets older? We can do this if we suppose that the atoms when they are first produced have not all the same strength of constitution, that some are more robust than others, perhaps because they contain more intrinsic energy to begin with, and will therefore have a longer life. Now if when the atoms are first produced there are some which will live for one year, some for ten, some for a thousand, and so on; and if lives of all durations, from nothing to infinity, are present in such proportion that the number of atoms which will live longer than a certain number of years decrease in a constant proportion for each additional year of life, we can easily prove that the expectation of life of an atom will be the same whatever its age may be. On this view the different atoms of a radio-active substance are not, in all respects, identical.

The energy developed by radio-active substances is exceedingly large, one gramme of radium developing nearly as much energy as would be produced by burning a ton of coal. This energy is mainly in the α particles, the positively charged helium atoms which are emitted when the change in the atom takes place; if this energy were produced by electrical forces it would indicate that the helium atom had moved through a potential difference of about two million volts on its way out of the atom of radium. The source of this energy is a problem of the deepest interest; if it arises from the repulsion of similarly electrified systems exerting forces varying inversely as the square of the distance, then to get the requisite amount of energy the systems, if their charges were comparable with the charge on the α particle, could not when they start be further apart than the radius of a corpuscle, 10^{-13} cm. If we suppose that the particles do not acquire this energy at the explosion, but that before they are shot out of the radium atom they move in circles

inside this atom with the speed with which they emerge, the forces required to prevent particles moving with this velocity from flying off at a tangent are so great that finite charges of electricity could only produce them at distances comparable with the radius of a corpuscle.

One method by which the requisite amount of energy could be obtained is suggested by the view to which I have already alluded—that in the atom we have electrified systems of very different types, one small, the other large; the radius of one type is comparable with 10^{-13} cm., that of the other is about 100,000 times greater. The electrostatic potential energy in the smaller bodies is enormously greater than that in the larger ones; if one of these small bodies were to explode and expand to the size of the larger ones, we should have a liberation of energy large enough to endow an α particle with the energy it possesses. Is it possible that the positive units of electricity were, to begin with, quite as small as the negative, but while in the course of ages most of these have passed from the smaller stage to the larger, there are some small ones still lingering in radio-active substances, and it is the explosion of these which liberates the energy set free during radio-active transformation?

The properties of radium have consequences of enormous importance to the geologist as well as to the physicist or chemist. In fact, the discovery of these properties has entirely altered the aspect of one of the most interesting geological problems, that of the age of the earth. Before the discovery of radium it was supposed that the supplies of heat furnished by chemical changes going on in the earth were quite insignificant, and that there was nothing to replace the heat which flows from the hot interior of the earth to the colder crust. Now when the earth first solidified it only possessed a certain amount of capital in the form of heat, and if it is continually spending this capital and not gaining any fresh heat it is evident that the process cannot have been going on for more than a certain number of years, otherwise the earth would be colder than it is. Lord Kelvin in this way estimated the age of the earth to be less than 100 million years. Though the quantity of radium in the earth is an exceedingly small fraction of the mass of the earth, only amounting, according to the determinations of Professors Strutt and Joly, to about five grammes in a cube whose side is 100 miles, yet the amount of heat given out by this small quantity of radium is so great that it is more than enough to replace the heat which flows from the inside to the outside of the earth. This, as Rutherford has pointed out, entirely vitiates the previous method of determining the age of the earth. The fact is that the radium gives out so much heat that we do not quite know what to do with it, for if there was as much radium throughout the interior of the earth as there is in its crust, the temperature of the earth would increase much more rapidly than it does as we descend below the

earth's surface. Professor Strutt has shown that if radium behaves in the interior of the earth as it does at the surface, rocks similar to those in the earth's crust cannot extend to a depth of more than forty-five miles below the surface.

It is remarkable that Professor Milne from the study of earthquake phenomena had previously come to the conclusion that rocks similar to those at the earth's surface only descend a short distance below the surface; he estimates this distance at about thirty miles, and concludes that at a depth greater than this the earth is fairly homogeneous.

Though the discovery of radio-activity has taken away one method of calculating the age of the earth it has supplied another.

The gas helium is given out by radio-active bodies, and since, except in beryls, it is not found in minerals which do not contain radio-active elements, it is probable that all the helium in these minerals has come from these elements. In the case of a mineral containing uranium, the parent of radium in radio-active equilibrium, with radium and its products, helium will be produced at a definite rate. Helium, however, unlike the radio-active elements, is permanent and accumulates in the mineral; hence if we measure the amount of helium in a sample of rock and the amount produced by the sample in one year we can find the length of time the helium has been accumulating, and hence the age of the rock. This method, which is due to Professor Strutt, may lead to determinations not merely of the average age of the crust of the earth, but of the ages of particular rocks and the date at which the various strata were deposited; he has, for example, shown in this way that a specimen of the mineral thorianite must be more than 240 million years old.

The physiological and medical properties of the rays emitted by radium is a field of research in which enough has already been done to justify the hope that it may lead to considerable alleviation of human suffering. It seems quite definitely established that for some diseases, notably rodent ulcer, treatment with these rays has produced remarkable cures; it is imperative, lest we should be passing over a means of saving life and health, that the subject should be investigated in a much more systematic and extensive manner than there has yet been either time or material for. Radium is, however, so costly that few hospitals could afford to undertake pioneering work of this kind; fortunately, however, through the generosity of Sir Ernest Cassel and Lord Iveagh a Radium Institute, under the patronage of his Majesty the King, has been founded in London for the study of the medical properties of radium, and for the treatment of patients suffering from diseases for which radium is beneficial.

The new discoveries made in physics in the last few years, and the ideas and potentialities suggested by them, have had an effect upon the

workers in that subject akin to that produced in literature by the Renaissance. Enthusiasm has been quickened, and there is a hopeful, youthful, perhaps exuberant, spirit abroad which leads men to make with confidence experiments which would have been thought fantastic twenty years ago. It has quite dispelled the pessimistic feeling, not uncommon at that time, that all the interesting things had been discovered, and all that was left was to alter a decimal or two in some physical constant. There never was any justification for this feeling, there never were any signs of an approach to finality in science. The sum of knowledge is at present, at any rate, a diverging not a converging series. As we conquer peak after peak we see in front of us regions full of interest and beauty, but we do not see our goal, we do not see the horizon; in the distance tower still higher peaks, which will yield to those who ascend them still wider prospects, and deepen the feeling, whose truth is emphasised by every advance in science, that 'Great are the Works of the Lord.'

British Association for the Advancement of Science.

SHEFFIELD, 1910.

ADDRESS.

BY

THE REV. PROFESSOR T. G. BONNEY, Sc.D., LL.D., F.R.S.,
PRESIDENT.

THIRTY-ONE years have passed since the British Association met in Sheffield, and the interval has been marked by exceptional progress. A town has become a city, the head of its municipality a Lord Mayor; its area has been enlarged by more than one-fifth; its population has increased from about 280,000 to 479,000. Communication has been facilitated by the construction of nearly thirty-eight miles of electric tramways for home service and of new railways, including alternative routes to Manchester and London. The supplies of electricity, gas, and water have more than kept pace with the wants of the city. The first was just being attempted in 1879; the second has now twenty-three times as many consumers as in those days; the story¹ of the third has been told by one who knows it well, so that it is enough for me to say your water supply cannot be surpassed for quantity and quality by any in the kingdom. Nor has Sheffield fallen behind other cities in its public buildings. In 1897 your handsome Town Hall was opened by the late Queen Victoria; the new Post Office, appropriately built and adorned with material from almost local sources, was inaugurated less than two months ago. The Mappin Art Gallery commemorates the munificence of those whose name it bears, and fosters that love of the beautiful which Ruskin sought to awaken by his generous gifts. Last, but not least, Sheffield has shown that it could not rest satisfied till its citizens could ascend from their own doors to the highest rung of the educational ladder. Firth College, named after its generous founder, was born in the year of our last visit; in 1897 it received a charter as the University College of Sheffield, and in the spring of 1905 was created a University, shortly after which its fine new buildings were

¹ *History and Description of Sheffield Water Works.* W. Terrey, 1908.

opened by the late King ; and last year its library, the generous gift of Dr. Edgar Allen, was inaugurated by his successor, when Prince of Wales. I must not now dwell on the great work which awaits this and other new universities. It is for them to prove that, so far from abstract thought being antagonistic to practical work, or scientific research to the labour of the factory or foundry, the one and the other can harmoniously co-operate in the advance of knowledge and the progress of civilisation.

You often permit your President on these occasions to speak of a subject in which he takes a special interest, and I prefer thus trespassing on your kindness to attempting a general review of recent progress in science. I do not, however, propose, as you might naturally expect, to discuss some branch of petrology ; though for this no place could be more appropriate than Sheffield, since it was the birthplace and the lifelong home of Henry Clifton Sorby, who may truly be called the father of that science. This title he won when, a little more than sixty years ago, he began to study the structure and mineral composition of rocks by examining thin sections of them under the microscope.¹ A rare combination of a singularly versatile and active intellect with accurate thought and sound judgment, shrewd in nature, as became a Yorkshireman, yet gentle, kindly, and unselfish, he was one whom his friends loved and of whom this city may well be proud. Sorby's name will be kept alive among you by the Professorship of Geology which he has endowed in your University ; but, as the funds will not be available for some time, and as that science is so intimately connected with metallurgy, coal-mining, and engineering, I venture to express a hope that some of your wealthier citizens will provide for the temporary deficiency, and thus worthily commemorate one so distinguished.

But to return. I have not selected petrology as my subject, partly because I think that the great attention which its more minute details have of late received has tended to limit rather than to broaden our views, while for a survey of our present position it is enough to refer to the suggestive and comprehensive volume published last year by Mr. A. Harker ;² partly, also, because the discussion of any branch of petrology would involve so many technicalities that I fear it would be found tedious by a large majority of my audience. So I have preferred to discuss some questions relating to the effects of ice which had engaged my attention a dozen years before I attempted the study of rock slices. As much of my petrological work has been connected with mountain

¹ His subsequent investigations into the microscopic structure of steel and other alloys of iron, in the manufacture of which your city holds a foremost place, have been extended by Mr. J. E. Stead and others, and they, besides being of great value to industrial progress, have thrown important sidelights on more than one dark place in petrology.

² *The Natural History of Igneous Rocks* (1909).

districts, it has been possible for me to carry on the latter without neglecting the former, and my study of ice-work gradually led me from the highlands into the lowlands.¹ I purpose, then, to ask your attention this evening to some aspects of the glacial history of Western Europe.

At no very distant geological epoch the climate in the northern part of the earth was much colder than it is at present. So it was also in the southern; but whether the two were contemporaneous is less certain. Still more doubtful are the extent and the work of the ice which was a consequence, and the origin of certain deposits on some northern lowlands, including those of our own islands: namely, whether they are the direct leavings of glaciers or were laid down beneath the sea by floating shore-ice and bergs. Much light will be thrown on this complex problem by endeavouring to ascertain what snow and ice have done in some region which, during the Glacial Epoch, was never submerged, and none better can be found for this purpose than the European Alps.

At the present day one school of geologists, which of late years has rapidly increased in number, claims for glaciers a very large share in the sculpture of that chain, asserting that they have not only scooped out the marginal lakes, as Sir A. Ramsay maintained full half a century ago, but have also quarried lofty cliffs, excavated great cirques, and deepened parts of the larger Alpine valleys by something like two thousand feet. The other school, while admitting that a glacier, under special circumstances, may hollow out a tarn or small lake and modify the features of rock scenery, declares that its action is abrasive rather than erosive, and that the sculpture of ridges, crags, and valleys was mainly accomplished in pre-glacial times by running water and the ordinary atmospheric agencies.

In all controversies, as time goes on, hypotheses are apt to masquerade as facts, so that I shall endeavour this evening to disentangle the two, and call attention to those which may be safely used in drawing a conclusion.

In certain mountain regions, especially those where strong limestones, granites, and other massive rocks are dominant, the valleys are often trench-like, with precipitous sides, having cirques or corries at their heads, and with rather wide and gently sloping floors, which occasionally descend in steps, the distance between these increasing with that from the watershed. Glaciers have unquestionably occupied many of these valleys, but of late years they have been supposed to have taken a large share in excavating them. In order to appreciate their action we must imagine the glens to be filled up and the district restored to its former condition of a more or less undulating upland. As the mean

¹ May I add that hereafter a statement of facts without mention of an authority means that I am speaking from personal knowledge.

temperature¹ declined snow would begin to accumulate in inequalities on the upper slopes. This, by melting and freezing, would soften and corrode the underlying material, which would then be removed by rain and wind, gravitation and avalanche. In course of time the hollow thus formed would assume more and more the outlines of a corrie or a cirque by eating into the hillside. With an increasing diameter it would be occupied, as the temperature fell, first by a permanent snowfield, then by the *névé* of a glacier. Another process now becomes important, that called 'sapping.' While ordinary glacier-scour tends, as we are told, to produce 'sweeping curves and eventually a graded slope,' 'sapping' produces 'benches and cliffs, its action being horizontal and backwards,' and often dominant over scour. The author of this hypothesis² convinced himself of its truth in the Sierra Nevada by descending a *bergschrand* 150 feet in depth, which opened out, as is so common, beneath the walls of a cirque. Beginning in the *névé*, it ultimately reached the cliff, so that for the last thirty feet the bold investigator found rock on the one hand and ice on the other. The former was traversed by fracture planes, and was in all stages of displacement and dislodgment; some blocks having fallen to the bottom, others bridging the narrow chasm, and others frozen into the *névé*. Clear ice had formed in the fissures of the cliff; it hung down in great stalactites; it had accumulated in stalagmitic masses on the floor. Beneath the *névé* the temperature would be uniform, so its action would be protective, except where it set up another kind of erosion, presently to be noticed; but in the chasm, we are informed, there would be, at any rate for a considerable part of the year, a daily alternation of freezing and thawing. Thus the cliff would be rapidly undermined and be carried back into the mountain slope, so that before long the glacier would nestle in a shelter of its own making. Farther down the valley the moving ice would become more effective than sub-glacial streams in deepening its bed; but since the *névé*-flow is almost imperceptible near the head, another agency must be invoked, that of 'plucking.' The ice grips, like a forceps, any loose or projecting fragment in its rocky bed, wrenches that from its place, and carries it away. The extraction of one tooth weakens the hold of its neighbours, and thus the glen is deepened by 'plucking,' while it is carried back by 'sapping.' Streams from melting snows on the slopes above the amphitheatre might have been expected to co-operate vigorously in making it, but of them little account seems to be taken, and we are even told that in some cases the winds probably prevented snow from resting on the rounded surface between two cirque-heads.³ As these receded,

¹ In the remainder of this Address 'temperature' is to be understood as mean temperature. The Fahrenheit scale is used.

² W. D. Johnson, *Science*, N.S., ix. (1899), pp. 106, 112.

³ This does not appear to have occurred in the Alps.

only a narrow neck would be left between them, which would be ultimately cut down into a gap or 'col.' Thus a region of deep valleys with precipitous sides and heads, of sharp ridges, and of more or less isolated peaks is substituted for a rather monotonous, if lofty, highland.

The hypothesis is ingenious, but some students of Alpine scenery think more proof desirable before they can accept it as an axiom. For instance, continuous observations are necessary to justify the assumption of diurnal variations of temperature sufficient to produce any sensible effect on rock at the bottom of a narrow chasm nearly fifty yards deep and almost enclosed by ice. Here the conditions would more probably resemble those in a *glacière*, or natural ice cave. In one of these, during the summer, curtains and festoons of ice depend from the walls; from them and from the roof water drips slowly, to be frozen into stalagmitic mounds on the floor, which is itself sometimes a thick bed of ice. On this the quantity of fallen rock *débris* is not greater than is usual in a cave, nor are the walls notably shattered, even though a gap some four yards deep may separate them from the ice. The floors of cirques, from which the *névé* has vanished, cannot as a rule be examined, because they are masked by *débris* which is brought down by the numerous cascades, little and big, which seam their walls; but glimpses of them may sometimes be obtained in the smaller corries (which would be cirques if they could), and these show no signs of either 'sapping' or 'plucking,' but some little of abrasion by moving ice. Cirques and corries also not infrequently occur on the sides as well as at the heads of valleys; such, for instance, as the two in the *massif* of the Uri Rothstock on the way to the Surenen Pass and the Fer à Cheval above Sixt. The Lago di Ritom lies between the mouth of a hanging valley and a well-defined step, and just above that is the Lago di Cadagno in a large, steep-walled corrie, which opens laterally into the Val Piora, as that of the Lago di Tremorgio does into the southern side of the Val Bedretto. Cirques may also be found where glaciers have had a comparatively brief existence, as the Creux des Vents on the Jura; or have never been formed, as on the slopes of Salina, one of the Lipari Islands, or in the limestone desert of Lower Egypt.¹ I have seen a miniature stepped valley carved by a rainstorm on a slope of Hampstead Heath; a cirque, about a yard in height and breadth, similarly excavated in the vertical wall of a gravel pit; and a corrie, measured by feet instead of furlongs, at the foot of one of the Binns near Burntisland, or, on a much reduced scale, in a bank of earth. On all these the same agent, plunging water, has left its marks—runlets of rain for the smaller, streams for the larger; convergent at first, perhaps, by accident,

¹ A. J. Jukes-Browne, *Geol. Mag.*, 1877, p. 477.

afterwards inevitably combined as the hollow widened and deepened. Each of the great cirques is still a 'land of streams,' and they are kept permanent for the greater part of the year by beds of snow on the ledges above its walls.

The 'sapping and plucking' process presents another difficulty—the steps already mentioned in the floors of valleys. These are supposed to indicate stages at which the excavating glacier transferred its operations to a higher level. But, if so, the outermost one must be the oldest, or the glacier must have been first formed in the lowest part of the incipient valley. Yet, with a falling temperature, the reverse would happen, for otherwise the snow must act as a protective mantle to the mature pre-glacial surface almost down to its base. However much age might have smoothed away youthful angularities, it would be strange if no receptacles had been left higher up to initiate the process; and even if sapping had only modified the form of an older valley, it could not have cut the steps unless it had begun its work on the lowest one. Thus, in the case of the Creux de Champ, if we hesitate to assume that the sapping process began at the mouth of the valley of the Grande Eau above Aigle, we must suppose it to have started somewhere near Ormont Dessus and to have excavated that gigantic hollow, the floor of which lies full 6,000 feet below the culminating crags of the Diablerets.

But even if 'sapping and plucking' were assigned a comparatively unimportant position in the cutting out of cirques and corries, it might still be maintained that the glaciers of the Ice Age had greatly deepened the valleys of mountain regions. That view is adopted by Professors Penck and Brückner in their work on the glaciation of the Alps,¹ the value of which even those who cannot accept some of their conclusions will thankfully admit. On one point all parties agree—that a valley cut by a fairly rapid stream in a durable rock is V-like in section. With an increase of speed the walls become more vertical; with a diminution the valley widens and has a flatter bed, over which the river, as the base-line is approached, may at last meander. Lateral streams will plough into the slopes, and may be numerous enough to convert them into alternating ridges and furrows. If a valley has been excavated in thick horizontal beds of rock varying in hardness, such as limestones and shales, its sides exhibit a succession of terrace walls and shelving banks, while a marked dip and other dominant structures produce their own modifications. It is also agreed that a valley excavated or greatly enlarged by a glacier should be U-like in section. But an Alpine valley, especially as we approach its head, very commonly takes the following form. For some hundreds of feet up from the torrent it is

¹ *Die Alpen in Eiszeitalter* (1909).

a distinct V; above this the slopes become less rapid, changing, say, from 45° to not more than 30° , and that rather suddenly. Still higher comes a region of stone-strewn upland valleys and rugged crags, terminating in ridges and peaks of splintered rock, projecting from a mantle of ice and snow. The V-like part is often from 800 to 1,000 feet in depth, and the above-named authors maintain that this, with perhaps as much of the more open trough above, was excavated during the Glacial Epoch. Thus the floor of any one of these valleys prior to the Ice Age must often have been at least 1,800 feet above its present level.¹ As a rough estimate we may fix the deepening of one of the larger Pennine valleys, tributary to the Rhone, to have been, during the Ice Age, at least 1,600 feet in their lower parts. Most of them are now hanging valleys; the stream issuing, on the level of the main river, from a deep gorge. Their tributaries are rather variable in form; the larger as a rule being more or less V-shaped; the shorter, and especially the smaller, corresponding more with the upper part of the larger valleys; but their lips generally are less deeply notched. Whatever may have been the cause, this rapid change in slope must indicate a corresponding change of action in the erosive agent. Here and there the apex of the V may be slightly flattened, but any approach to a real U is extremely rare. The retention of the more open form in many small, elevated recesses, from which at the present day but little water descends, suggests that where one of them soon became buried under snow,² but was insignificant as a feeder of a glacier, erosion has been for ages almost at a standstill.

The V-like lower portion in the section of one of the principal valleys, which is all that some other observers have claimed for the work of a glacier, cannot be ascribed to subsequent modification by water, because ice-worn rock can be seen in many places, not only high up its sides, but also down to within a yard or two of the present torrent.

Thus valley after valley in the Alps seems to leave no escape from the following dilemma: Either a valley cut by a glacier does not differ in form from one made by running water, or one which has been excavated by the latter, if subsequently occupied, is but superficially modified by ice. This, as we can repeatedly see in the higher Alpine valleys, has not succeeded in obliterating the physical features due to the ordinary processes of erosion. Even where its effects are most striking, as

¹ The amount varies in different valleys; for instance, it was fully 2,880 feet at Amsteg on the Reuss, just over 2,000 feet at Brieg in the Rhone Valley, about 1,000 feet at Guttanen in the Aare Valley, about 1,550 feet above Zermatt, and 1,100 feet above Saas Grund.

² My own studies of mountain districts have led me to infer that on slopes of low grade the action of snow is preservative rather than destructive. That conclusion was confirmed by Professor Garwood in a communication to the Royal Geographical Society on June 20 of the present year.

in the Spitalamm below the Grimsel Hospice, it has not wholly effaced those features; and wherever a glacier in a recent retreat has exposed a rock surface, that demonstrates its inefficiency as a plough. The evidence of such cases has been pronounced inadmissible, on the ground that the glaciers of the Alps have now degenerated into senile impotence; but in valley beds over which they passed when in the full tide of their strength the flanks show remnants of rocky ridges only partly smoothed away, and rough rock exists on the 'lee-sides' of ice-worn mounds which no imaginary plucking can explain. The ice seems to have flowed over rather than to have plunged into the obstacles in its path, and even the huge steps of limestone exposed by the last retreat of the Unter Grindelwald Glacier have suffered little more than a rounding off of their angles, though that glacier must have passed over them when in fullest development, for it seems impossible to explain these by any process of sapping.

The comparatively level trough, which so often forms the uppermost part of one of the great passes across the watershed of the Alps, can hardly be explained without admitting that in each case the original watershed has been destroyed by the more rapid recession of the head of the southern valley, and this work bears every sign of having been accomplished in pre-glacial times. Sapping and plucking must have operated on a gigantic scale to separate the Viso from the Cottian watershed, to isolate the huge pyramid of the Matterhorn, with its western spur, or to make, by the recession of the Val Macugnaga, that great gap between the Stralhorn and Monte Rosa. Some sceptics even go so far as to doubt whether the dominant forms of a non-glaciated region differ very materially from those of one which has been half-buried in snow-fields and glaciers. To my eyes, the general outlines of the mountains about the Lake of Gennesaret and the northern part of the Dead Sea recalled those around the Lake of Annecy and on the south-eastern shore of Lemman. The sandstone crags, which rise here and there like ruined castles from the lower plateau of the Saxon Switzerland, resembled in outlines, though on a smaller scale, some of the Dolomites in the Southern Tyrol. The Lofoten Islands illustrate a half-drowned mountain range from which the glaciers have disappeared. Those were born among splintered peaks and ridges, which, though less lofty, rival in form the Aiguilles of Chamonix, and the valleys become more and more ice-worn as they descend, till the coast is fringed with skerries every one of which is a *roche moutonnée*. The *névé* in each of these valleys has been comparatively ineffective; the ice has gathered strength with the growth of the glacier. As can be seen from photographs, the scenery of the heart of the Caucasus or of the Himalayas differs in scale rather than in kind from that of the Alps. Thus the amount of abrasion varies, other things

being equal, with the latitude. The grinding away of ridges and spurs, the smoothing of the walls of troughs,¹ is greater in Norway than in the Alps; it is still greater in Greenland than in Norway, and it is greatest of all in the Antarctic, according to the reports of the expeditions led by Scott and Shackleton. But even in Polar regions, under the most favourable conditions, the dominant outlines of the mountains, as shown in the numerous photographs taken by both parties, and in Dr. Wilson's admirable drawings, differ in degree rather than in kind from those of mid-European ranges. It has been asserted that the parallel sides of the larger Alpine valleys—such as the Rhone above Martigny, the Lutschine near Lauterbrunnen, and the Val Bedretto below Airolo—prove that they have been made by the ice-plough rather than by running water; but in the first I am unable to discern more than the normal effects of a rather rapid river which has followed a trough of comparatively soft rocks; in the second, only the cliffs marking the channel cut by a similar stream through massive limestones—cliffs like those which elsewhere rise up the mountain flanks far above the levels reached by glaciers; while in the third I have failed to discover, after repeated examination, anything abnormal.

Many lake basins have been ascribed to the erosive action of glaciers. Since the late Sir A. Ramsay advanced this hypothesis numbers of lakes in various countries have been carefully investigated and the results published, the most recent of which is the splendid work on the Scottish lochs by Sir J. Murray and Mr. L. Pullar.² A contribution to science of the highest value, it has also a deeply pathetic interest, for it is a father's memorial to a much-loved son, F. P. Pullar, who, after taking a most active part in beginning the investigation, lost his life while saving others from drowning. As the time at my command is limited, and many are acquainted with the literature of the subject, I may be excused from saying more than that even these latest researches have not driven me from the position which I have maintained from the first—namely, that while many tarns in corries and lakelets in other favourable situations are probably due to excavation by ice, as in the mountainous districts of Britain, in Scandinavia, or in the higher parts of the Alps, the difficulty of invoking this agency increases with the size of the basin—as, for example, in the case of Loch Maree or the Lake of Annecy—till it becomes insuperable. Even if Glas Llyn and Llyn Llydaw were the work of a glacier, the rock basins of Gennesaret and the Dead Sea, still more those of the great lakes in North America and in Central Africa, must be assigned to other causes.

¹ If one may judge from photographs, the smoothing of the flanks of a valley is unusually conspicuous in Milton Sound, New Zealand.

² *Bathymetrical Survey of the Scottish Freshwater Lochs*. Sir J. Murray and Mr. L. Pullar, 1910.

I pass on, therefore, to mention another difficulty in this hypothesis—that the Alpine valleys were greatly deepened during the Glacial Epoch—which has not yet, I think, received sufficient attention. From three to four hundred thousand years have elapsed, according to Penck and Brückner, since the first great advance of the Alpine ice. One of the latest estimates of the thickness of the several geological formations assigns 4,000 feet¹ to the Pleistocene and Recent, 13,000 to the Pliocene, and 14,000 to the Miocene. If we assume the times of deposit to be proportional to the thicknesses, and adopt the larger figure for the first-named period, the duration of the Pliocene would be 1,300,000 years, and of the Miocene 1,400,000 years. To estimate the total vertical thickness of rock which has been removed from the Alps by denudation is far from easy, but I think 14,000 feet would be a liberal allowance, of which about one-seventh is assigned to the Ice Age. But during that age, according to a curve given by Penck and Brückner, the temperature was below its present amount for rather less than half (.47) the time. Hence it follows that, since the sculpture of the Alps must have begun at least as far back as the Miocene period, one-seventh of the work has been done by ice in not quite one-fifteenth of the time, or its action must be very potent. Such data as are at our command make it probable that a Norway glacier at the present day lowers its basin by only about eighty millimètres in 1,000 years; a Greenland glacier may remove some 421 millimètres in the same time, while the Vatnajökul in Iceland attains to 647 millimètres. If Alpine glaciers had been as effective as the last-named, they would not have removed, during their 188,000 years of occupation of the Alpine valleys, more than 121.6 metres, or just over 397 feet; and as this is not half the amount demanded by the more moderate advocates of erosion, we must either ascribe an abnormal activity to the vanished Alpine glaciers, or admit that water was much more effective as an excavator.

We must not forget that glaciers cannot have been important agents in the sculpture of the Alps during more than part of Pleistocene times. That sculpture probably began in the Oligocene period; for rather early in the next one the great masses of conglomerate, called *Nagelfluh*, show that powerful rivers had already carved for themselves valleys corresponding generally with and nearly as deep as those still in existence. Temperature during much of the Miocene period was not less than 12° F. above its present average. This would place the snow-line at about 12,000 feet.²

¹ I have doubts whether this is not too great.

² I take the fall of temperature for a rise in altitude as 1° F. for 300 feet or, when the differences in the latter are large, 3° per 1,000 feet. These estimates will, I think, be sufficiently accurate. The figures given by Hann (see for a discussion of the question, *Report of Brit. Assoc.*, 1909, p. 93) work out to 1° F. for each 318 feet of ascent (up to about 10,000 feet).

In that case, if we assume the altitudes unchanged, not a snowfield would be left between the Simplon and the Maloja, the glaciers of the Pennines would shrivel into insignificance, Monte Rosa would exchange its drapery of ice for little more than a tippet of frozen snow. As the temperature fell the white robes would steal down the mountain-sides, the glaciers grow, the torrents be swollen during all the warmer months, and the work of sculpture increase in activity. Yet with a temperature even 6° higher than it now is, as it might well be at the beginning of the Pliocene period, the snow-line would be at 10,000 feet; numbers of glaciers would have disappeared, and those around the Jungfrau and the Finster Aarhorn would be hardly more important than they now are in the Western Oberland.

But denudation would begin so soon as the ground rose above the sea. Water, which cannot run off the sand exposed by the retreating tide without carving a miniature system of valleys, would never leave the nascent range intact. The Miocene Alps, even before a patch of snow could remain through the summer months, would be carved into glens and valleys. Towards the end of that period the Alps were affected by a new set of movements, which produced their most marked effects in the northern zone from the Inn to the Durance. The Oberland rose to greater importance; Mont Blanc attained its primacy; the *massif* of Dauphiné was probably developed. That, and still more the falling temperature, would increase the snowfields, glaciers, and torrents. The first would be, in the main, protective; the second, locally abrasive; the third, for the greater part of their course, erosive. No sooner had the drainage system been developed on both sides of the Alps than the valleys on the Italian side (unless we assume a very different distribution of rainfall) would work backwards more rapidly than those on the northern. Cases of trespass, such as that recorded by the long level trough on the north side of the Maloja Kulm and the precipitous descent on the southern, would become frequent. In the interglacial episodes—three in number, according to Penck and Brückner, and occupying rather more than half the epoch—the snow and ice would dwindle to something like its present amount, so that the water would resume its work. Thus I think it far more probable that the V-like portions of the Alpine valleys were in the main excavated during Pliocene ages, their upper and more open parts being largely the results of Miocene and yet earlier sculpture.

During the great advances of the ice, four in number, according to Penck and Brückner,¹ when the Rhone glacier covered the lowlands of Vaud and Geneva, welling on one occasion over the gaps in the Jura, and leaving its erratics in the neighbourhood of Lyons, it ought to have

¹ On the exact number I have not had the opportunity of forming an opinion.

given signs of its erosive no less than of its transporting power. But what are the facts? In these lowlands we can see where the ice has passed over the Molasse (a Miocene sandstone); but here, instead of having crushed, torn, and uprooted the comparatively soft rock, it has produced hardly any effect. The huge glacier from the Linth Valley crept for not a few miles over a floor of stratified gravels, on which, some eight miles below Zurich, one of its moraines, formed during the last retreat, can be seen resting, without having produced more than a slight superficial disturbance. We are asked to credit glaciers with the erosion of deep valleys and the excavation of great lakes, and yet, wherever we pass from hypotheses to facts, we find them to have been singularly inefficient workmen!

I have dwelt at considerable, some may think undue, length on the Alps, because we are sure that this region from before the close of the Miocene period has been above the sea-level. It accordingly demonstrates what effects ice can produce when working on land.

In America also, to which I must now make only a passing reference, great ice-sheets formerly existed: one occupying the district west of the Rocky Mountains, another spreading from that on the north-west of Hudson's Bay, and a third from the Laurentian hill-country. These two became confluent, and their united ice-flow covered the region of the Great Lakes, halting near the eastern coast a little south of New York, but in Ohio, Indiana, and Illinois occasionally leaving moraines only a little north of the 39th parallel of latitude.¹ Of these relics my first-hand knowledge is very small, but the admirably illustrated reports and other writings of American geologists² indicate that, if we make due allowance for the differences in environment, the tills and associated deposits on their continent are similar in character to those of the Alps.³

In our own country and in corresponding parts of Northern Europe we must take into account the possible co-operation of the sea. In these, however, geologists agree that, for at least a portion of the Ice Age, glaciers occupied the mountain districts. Here ice-worn rocks, moraines and perched blocks, tarns in corries, and perhaps lakelets in valleys, demonstrate the former presence of a mantle of snow and ice. Glaciers radiated outwards from more than one focus in Ireland, Scotland, the English Lake District, and Wales, and trespassed, at the time

¹ Some of the glacial drifts on the eastern side of the continent, as we shall find, may have been deposited in the sea.

² See the *Reports of the United States Geological Survey* (from vol. iii. onwards), *Journal of Geology*, *American Journal of Science*, and local publications too numerous to mention. Among these the studies in Greenland by Professor Chamberlin are especially valuable for the light they throw on the movement of large glaciers and the transport of *débris* in the lower part of the ice.

³ Here, however, we cannot always be so sure of the absence of the sea.

of their greatest development, upon the adjacent lowlands. They are generally believed to have advanced and retreated more than once, and their movements have been correlated by Professor J. Geikie with those already mentioned in the Alps. Into that very difficult question I must not enter; for my present purpose it is enough to say that in early Pleistocene times glaciers undoubtedly existed in the mountain districts of Britain and even formed piedmont ice-sheets on the lowlands. On the west side of England, smoothed and striated rocks have been observed near Liverpool, which can hardly be due to the movements of shore-ice, and at Little Crosby a considerable surface has been cleared from the overlying boulder clay by the exertions of the late Mr. T. M. Reade and his son, Mr. A. Lyell Reade. But, so far as I am aware, rocks thus affected have not yet been discovered in the Wirral peninsula. On the eastern side of England similar markings have been found down to the coast of Durham, but a more southern extension of land ice cannot be taken for granted. In this direction, however, so far as the tidal valley of the Thames, and in corresponding parts of the central and western lowlands, certain deposits occur which, though to a great extent of glacial origin, are in many respects different from those left by land ice in the Alpine regions and in Northern America.

They present us with problems the nature of which may be inferred from a brief statement of the facts. On the Norfolk coast we find the glacial drifts resting, sometimes on the chalk, sometimes on strata of very late Pliocene or early Pleistocene age. The latter show that in their time the strand-line must have oscillated slightly on either side of its present level. The earliest of the glacial deposits, called the Cromer Till and Contorted Drift, presents its most remarkable development in the cliffs on either side of that town. Here it consists of boulder clays and alternating beds of sand and clay; the first-named, two or three in number, somewhat limited in extent, and rather lenticular in form, are slightly sandy clays, full of pieces of chalk, flint, and other kinds of rock, some of the last having travelled from long distances. Yet more remarkable are the huge erratics of chalk, in the neighbourhood of which the sands and clays exhibit extraordinary contortions. Like the beds of till, they have not been found very far inland, for there the group appears as a whole to be represented by a stony loam, resembling a mixture of the sandy and clayey material, and this is restricted to a zone some twenty miles wide, bordering the coast of Norfolk and Suffolk; not extending south of the latter county, but being probably represented to the north of the Humber. Above these is a group of false-bedded sands and gravels, variable in thickness and character—the Mid-glacial Sands of Searles V. Wood and F. W. Harmer. They extend over a wider area,

and may be traced, according to some geologists, nearly to the western side of England, rising in that direction to a greater height above sea-level. But as it is impossible to prove that all isolated patches of these materials are identical in age, we can only be certain that some of them are older than the next deposit, a boulder clay, which extends over a large part of the lowlands in the Eastern Counties. This has a general resemblance to the Cromer Till, but its matrix is rather more clayey and is variable in colour. In and north of Yorkshire, as well as on the seaward side of the Lincolnshire wolds, it is generally brownish or purplish, but on their western side and as far as the clay goes to the south it is some shade of grey. Near to these wolds, in mid-Norfolk, and on the northern margin of Suffolk, it has a whitish tint, owing to the abundance of comminuted chalk. To the south and west of this area it is dark, from the similar presence of Kimeridge clay. Yet further west it assumes an intermediate colour by having drawn upon the Oxford clay. This boulder clay, whether the chalky or the purple, in which partings of sand sometimes occur, must once have covered, according to Mr. F. W. Harmer, an area about ten thousand square miles in extent. It spreads like a coverlet over the pre-glacial irregularities of the surface. It caps the hills, attaining sometimes an elevation of fully 500 feet above sea-level;¹ it fills up valleys,² sometimes partly, sometimes wholly, the original floors of which occasionally lie more than 100 feet below the same level. This boulder clay, often with an underlying sand or gravel, extends to the south as far as the neighbourhood of Muswell Hill and Finchley; hence its margin runs westward through Buckinghamshire, and then, bending northwards, passes to the west of Coventry. On this side of the Pennine Chain the matrix of the boulder clay is again reddish, being mainly derived from the sands and marls of the Trias; pieces of chalk and flint are rare (no doubt coming from Antrim), though other rocks are often plentiful enough. Some authorities are of opinion that the drift in most parts of Lancashire and Cheshire is separable, as on the eastern coasts, into a lower and an upper boulder clay, with intervening gravelly sands, but others think that the association of the first and third is lenticular rather than suc-

¹ Not far from Royston it is found at a height of 525 feet above O.D. See F. W. Harmer, *Pleistocene Period in the Eastern Counties*, p. 115.

² At Old North Road Station, on a tributary of the Cam, the boulder clay was pierced to a depth of 180 feet, and at Impington it goes to 60 feet below sea-level. Near Hitchin, a hidden valley, traced for seven or eight miles, was proved to a depth of 68 feet below O.D., and one near Newport in Essex, to 140 feet. Depths were also found of 120 feet at West Horseheath in Suffolk, of 120 feet on low ground two miles S.W. of Sandy in Bedfordshire, of from 100 to 160 feet below the sea at Fossdyke, Long Sutton, and Boston, and at Glemsford in the valley of the Stour, 477 feet of drift was passed through before reaching the chalk. See F. W. Harmer, *Quart. Journ. Geol. Soc.*, lxiii. (1907), p. 494.

cessive. Here also the lower clay cannot be traced very far inland, eastward or southward; the others have a wider extension, but they reach a greater elevation above sea-level than on the eastern side of England. The sand is inconstant in thickness, being sometimes hardly represented, sometimes as much as 200 feet. The upper clay runs on its more eastern side up to the chalky boulder clay, and extends on the south at least into Worcestershire. On the western side it merges with the upper member of the drifts radiating from the mountains of North Wales, which often exhibit a similar tripartite division, while (as we learn from the officers of the Geological Survey) boulder clays and gravelly sands, which it must suffice to mention, extend from the highlands of South Wales for a considerable distance to the south-east and south. Boulder clay has not been recognised in Devon or Cornwall, though occasional erratics are found which seem to demand some form of ice-transport. A limited deposit, however, of that clay, containing boulders now and then over a yard in diameter, occurs near Selsey Bill on the Sussex coast, which most geologists consider to have been formed by floating rather than by land ice.

Marine shells are not very infrequent in the lower clays of East Anglia and Yorkshire, but are commonly broken. The well-known Bridlington Crag is the most conspicuous instance, but this is explained by many geologists as an erratic—a piece of an ancient North Sea bed caught up and transported, like the other molluscs, by an advancing ice-sheet. They also claim a derivative origin for the organic contents of the overlying sands and gravels, but some authorities consider the majority to be contemporaneous. Near the western coast of England, shells in much the same state of preservation as those on the present shore are far from rare in the lower clay, where they are associated with numerous striated stones, often closely resembling those which have travelled beneath a glacier, both from the Lake District and the less distant Trias. Shells are also found in the overlying sands up the valleys of the Dee and Severn, at occasional localities, even as far inland as Bridgnorth, the heights of the deposits varying from about 120 feet to over 500 feet above the sea-level. If we also take account of the upper boulder clay, where it can be distinguished, the list of marine molluscs, ostracods, and foraminifers from these western drifts is a rather long one.¹

Marine shells, however, on the western side of England, are not restricted to the lowlands. Three instances, all occurring over 1,000 feet above sea-level, claim more than a passing mention. At Macclesfield, almost thirty miles in a straight line from the head of the estuary of the Mersey, boulder clays associated with stratified gravels

¹ W. Shone, *Quart. Journ. Geol. Soc.*, xxxiv. (1878), p. 323.

and sands have been described by several observers.¹ The clay stops at about 1,000 feet, but the sands and gravels go on to nearly 1,300 feet, while isolated erratics are found up to about 100 feet higher. Sea shells, some of which are in good condition, have been obtained at various elevations, the highest being about 1,200 feet above sea-level. About forty-eight species of molluscs have been recognised, and the fauna, with a few exceptions, more arctic in character and now found at a greater depth, is one which at the present day lives in a temperate climate at a depth of a few fathoms.

The shell-bearing gravels at Gloppa, near Oswestry, which are about thirty miles from the head of the Dee estuary, were carefully described in 1892 by Mr. A. C. Nicholson. He has enumerated fully sixty species, of which, however, many are rare. As his collection² shows, the bivalves are generally broken, but a fair number of the univalves are tolerably perfect. The deposit itself consists of alternating seams of sand and gravel, the one generally about an inch in thickness, the other varying from a few inches to a foot. The difference in the amount of rounding shown by the stones is a noteworthy feature. They are not seldom striated; some have come from Scotland, others from the Lake District, but the majority from Wales, the last being the more angular. Here and there, a block, sometimes exceeding a foot in diameter and usually from the last-named country, has been dropped among the smaller material, most of which ranges in diameter from half an inch to an inch and a half. The beds in one or two places show contortions; but as a rule, though slightly wavy and with a gentle dip rather to the west of south, they are uniformly deposited. In this respect, and in the unequal wearing of the materials, the Gloppa deposit differs from most gravels that I have seen. Its situation also is peculiar. It is on the flattened top of a rocky spur from higher hills, which falls rather steeply to the Shropshire lowland on the eastern side, and on the more western is defined by a small valley which enlarges gradually as it descends towards the Severn. If the country were gradually depressed for nearly 1,200 feet, this upland would become, first a promontory, then an island, and finally a shoal.

The third instance, on Moel Tryfaen in Carnarvonshire, was carefully investigated and described by a Committee of this Association³ about ten years ago. The shells occur in an irregularly stratified sand and gravel, resting on slate, and overlain by a boulder clay, no great

¹ *Memoirs of the Geological Survey*: 'Country around Macclesfield,' T. I. Pocock (1906), p. 85. For some notes on Moel Tryfaen and the altitudes of other localities at which marine organisms have been found see J. Gwyn Jeffreys, *Quart. Journ. Geol. Soc.*, xxxvi. (1880), p. 351. For the occurrence of such remains in the Vale of Clwyd see a paper by T. McK. Hughes in *Proc. Chester Soc. of Nat. Hist.*, 1884.

² Now deposited in the Oswestry Museum.

³ *Brit. Assoc. R. port*, 1899 (1900), pp. 414-423.

distance from and a few dozen feet below the rocky summit of the hill, being about 1,300 feet above the level of the sea and at least five miles from its margin. About fifty-five species of molluscs and twenty-three of foraminifers have been identified. According to the late Dr. J. Gwyn Jeffreys,¹ the majority of the molluscs are littoral in habit, the rest such as live in from ten to twenty fathoms of water. Most of the erratics have been derived from the Welsh mountains, but some rocks from Anglesey have also been obtained, and a few pebbles of Lake District and Scotch rocks. If the sea were about 1,300 feet above its present level, Moel Tryfaen would become a small rocky island, open to the storms from the west and north, and nearly a mile and a half away from the nearest land.

I must pass more rapidly over Ireland. The signs of vanished glaciers—ice-worn rocks and characteristic boulder-clays—are numerous, and may be traced in places down to the sea-level, but the principal outflow of the ice, according to some competent observers, was from a comparatively low district, extending diagonally across the island from the south of Lough Neagh to north of Galway Bay. Glaciers, however, must have first begun to form in the mountains on the northern and southern side of this zone, and we should have expected that, whatever might happen on the lowlands, they would continue to assert themselves. In no other part of the British Islands are eskers, which some geologists think were formed when a glacier reached the sea, so strikingly developed. Here also an upper and a lower boulder clay, the former being the more sparsely distributed, are often divided by a widespread group of sands and gravels, which locally, as in Great Britain, contains, sometimes abundantly, shells and other marine organisms; more than twenty species of molluscs, with foraminifers, a barnacle, and perforations of annelids, having been described. These are found in counties Dublin and Wicklow, at various altitudes,² from a little above sea-level to a height of 1,300 feet.

Not the least perplexing of the glacial phenomena in the British Isles is the distribution of erratics, which has been already mentioned in passing. On the Norfolk coast, masses of chalk, often thousands of cubic feet in volume, occur in the lowest member of the glacial series, with occasional great blocks of sand and gravel, which must have once been frozen. But these, or at any rate the larger of them, have no doubt been derived from the immediate neighbourhood. Huge erratics also occasionally occur in the upper boulder clay—sometimes of chalk, as at Roslyn Hill near Ely and at Ridlington in Rutland, of jurassic limestone, near Great Ponton, to the south of Grantham,

¹ *Quart. Journ. Geol. Soc.*, xxxvi. (1880), p. 355.

² See T. M. Reade, *Proc. Liverpool Geol. Soc.*, 1893-94, p. 183, for some weighty arguments in favour of a marine origin for these deposits.

and of Lower Kimeridge clay near Biggleswade.¹ These also probably have not travelled more than a few miles. But others of smaller size have often made much longer journeys. The boulder clays of Eastern England are full of pieces of rock, commonly ranging from about half an inch to a foot in diameter. Among these are samples of the carboniferous, jurassic, and cretaceous rocks of Yorkshire and the adjacent counties; the red chalk from either Hunstanton, Speeton, or some part of the Lincolnshire wolds, being found as far south as the northern heights of London. Even the chalk and flint, the former of which, especially in the upper boulder clay, commonly occurs in well-worn pebbles, are frequently not the local but the northern varieties. And with these are mingled specimens from yet more distant sources—Cheviot porphyrites, South Scotch basalts, even some of the crystalline rocks of the Highlands. Whatever was the transporting agent, its general direction was southerly, with a slight deflection towards the east in the last-named cases.

But the path of these erratics has been crossed by two streams, one coming from the west, the other from the east. On the western side of the Pennine watershed the Shap granite rises at Wasdale Crag to a height of about 1,600 feet above sea-level. Boulders from it have descended the Eden valley to beyond Penrith; they have travelled in the opposite direction almost to Lancaster,² and a large number of them have actually made their way near the line of the Lake District watershed, across the upper valley of the Eden, and over the high pass of Stainmoor Forest,³ whence they descended into Upper Teesdale. Subsequently the stream seems to have bifurcated, one part passing straight out to the present sea-bed, by way of the lower course of the Tees, to be afterwards driven back on to the Yorkshire coast. The other part crossed the low watershed between the Tees and the Ouse, descended the Vale of York and spread widely over the plain.⁴ Shap boulders by some means penetrated into the valleys tributary to the Ouse on its west bank, and they have been observed as far to the south-east as Royston, near Barnsley. It is noteworthy that Lake District rocks have been occasionally recorded from Airedale and even the neighbourhood of Colne, though the granite from Shap has not been found there. The other stream started from Scandinavia. Erratics, some of which must have come from the north-western side of the Christiania Fjord, occur on or near the coast from Essex to Yorkshire, and occasionally

¹ H. Home, *Quart. Journ. Geol. Soc.*, lix. (1903), p. 375.

² A pebble of it is said to have been identified at Moel Tryfaen.

³ The lowest part of the gap is about 1,400 feet. A little to the south is another gap about 200 feet lower, but none of the boulders seem to have taken that route.

⁴ A boulder was even found above Grosmont in the Eske valley, 345 feet above sea-level.

even as far north as Aberdeen, while they have been traced from the East Anglian coast to near Ware, Hitchin, and Bedford.¹ It may be important to notice that these Scandinavian erratics are often waterworn, like those dispersed over Denmark and parts of Northern Germany.

On the western side of England the course of erratics is not less remarkable. Boulders from South-Western Scotland, especially from the Kirkcudbright district, both waterworn and angular, are scattered over the lowlands as far south as Wolverhampton, Bridgnorth, and Church Stretton. They may be traced along the border of North Wales, occurring, as has been said, though generally small, up to about 1,300 feet on Moel Tryfaen, 1,100 feet at Gloppa, and more than that height on the hills east of Macclesfield. Boulders from the Lake District are scattered over much the same area and attain the same elevation, but extend, as might be expected, rather farther to the east in Lancashire. They also have been found on the eastern side of the Pennine watershed, perhaps the most remarkable instances being in the dales of the Derbyshire Derwent and on the adjacent hills as much as 1,400 feet above the sea-level.² A third remarkable stream of erratics from the neighbourhood of the Arenig mountains extends from near the estuary of the Dee right across the paths of the two streams from the north, its eastern border passing near Rugeley, Birmingham, and Bromsgrove. They also range high, occurring almost 900 feet above sea-level on Romsley Hill, north of the Clents, and being common at Gloppa. Boulders also from the basalt mass of Rowley Regis have travelled in some cases between four and five miles, and in directions ranging from rather west of south to north-east; and, though that mass hardly rises above the 700-feet contour line, one lies with an Arenig boulder on Romsley Hill. From Charnwood Forest, the crags of which range up to about 850 feet above sea-level, boulders have started which have been traced over an area to the south and west to a distance of more than twenty miles.

Such, then, are the facts, which call for an interpretation. More than one has been proposed; but it will be well, before discussing them, to arrive at some idea of the climate of these islands during the colder part of the Glacial Epoch. Unless that were associated with very great changes in the distribution of sea and land in Northern and North-Western Europe, we may assume that neither the relative position of the isotherms nor the distribution of precipitation would be very materially altered. A general fall of temperature in the northern hemisphere might so weaken the warmer ocean current from the south-west that our coasts might be approached by a cold one from the

¹ R. H. Rastall and J. Romanes, *Quart. Journ. Geol. Soc.*, lxx. (1909), p. 246.

² Communication from Dr. H. Arnold-Bemrose.

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opposite direction.¹ But though these changes might diminish the difference between the temperatures of London and Leipzig, they would not make the former colder than the latter. At the present day the snow-line in the Alps on either side of the Upper Rhone Valley is not far from 8,000 feet above sea-level, and this corresponds with a temperature of about 30°. Glaciers, however, are not generally formed till about 1,000 feet higher, where the temperature is approximately 27°. Penck and Brückner place this line during the coldest part of the Ice Age at about 4,000 feet.² In that case the temperature of the Swiss lowland would be some 15° lower than now, or near the freezing point.³ If this fall were general, it would bring back the small glaciers on the Gran Sasso d'Italia and Monte Rotondo in Corsica; perhaps also among the higher parts of the Vosges and Schwarzwald.⁴ In our own country it would give a temperature of about 35° at Carnarvon and 23° on the top of Snowdon, of 32° at Fort William and 17.5° on the top of Ben Nevis. If, in addition to this, the land were 600 feet higher than now (as it probably was, at any rate in the beginning of the Glacial Epoch), there would be a further drop of 2°, so that glaciers would form in the corries of Snowdon, and the region round Ben Nevis might resemble the Oetzthal Alps at the present day. This change of itself would be insufficient, and any larger drop in the ocean-level would have to be continental in its effects, since we cannot assume a local upheaval of much more than the above amount without seriously interfering with the river system of North Central Europe. But these changes, especially the former, might indirectly diminish the abnormal warmth of winter on our north-western coasts.⁵ It is difficult to estimate the effect of this. If it did no more than place Carnarvon on the isotherm of Berlin (now lower by 2°), that would hardly bring a glacier from the Snowdonian region down to the sea. At the present time London is about 18° warmer than a place in the same latitude near the Labrador coast or the mouth of the Amur River, but the removal of that difference would involve greater changes in the distribution of sea and land than seems possible at an epoch comparatively speaking so recent.

¹ Facts relating to this subject will be found in *Climate and Time*, by J. Croll, ch. ii. and iii. (1875). Of course the air currents would also be affected, and perhaps diminish precipitation as the latitude increased.

² *Loc. cit.* p. 580, *et seq.* They say the snow-line, which would mean that the temperature was only 12° lower than now; but as possibly this line might then more nearly correspond with that of glacier formation, I will provisionally accept the higher figures, especially since Corsica, the Apennines, and some other localities in Europe, seem to require a reduction of rather more than 12°.

³ It would be 32.5° at Zurich, 31.6° at Bern, 34.1° at Geneva, about 39.0° on the plain of Piedmont, and 36.0° at Lyons.

⁴ See for particulars the author's *Ice Work* ('International Scientific Series'), p. 237.

⁵ For much valuable information on these questions see a paper on the Climate of the Pleistocene Epoch (F. W. Harmer, *Quart. Journ. Geol. Soc.*, lvii. (1901), p. 405).

I am doubtful whether we can attribute to changed currents a reduction in British temperatures of so much as 11° ; but, if we did, this would amount to 28° from all causes, and give a temperature of 20° to 22° at sea-level in England, during the coldest part of the Glacial Epoch.¹ That is now found, roughly speaking, in Spitzbergen, which, since its mountains rise to much the same height, should give us a general idea of the condition of Britain in the olden time.

What would then be the state of Scandinavia? Its present temperature ranges on the west coast from about 45° in the south to 35° in the north.² But this region must now be very much, possibly 1,800 feet, lower than it was in pre-glacial, perhaps also in part of glacial, times.³ If we added 5° for this to the original 15° , and allowed so much as 18° for the diversion of the warm current, the temperature of Scandinavia would range from 7° to -3° , approximately that of Greenland northwards from Upernivik. But since the difference at the present day between Cape Farewell and Christiania (the one in an abnormally cold region, the other in one correspondingly warm) is only 7° , that allowance seems much too large, while without it Scandinavia would correspond in temperature with some part of that country from south of Upernivik to north of Frederikshaab.⁴ But if Christiania were not colder than Jakobshavn is now, or Britain than Spitzbergen, we are precluded from comparisons with the coasts of Baffin Bay or Victoria Land.

Thus the ice-sheet from Scandinavia would probably be much greater than those generated in Britain. It would, however, find an obstacle to progress westwards, which cannot be ignored. If the bed of the North Sea became dry land, owing to a general rise of 600 feet, that would still be separated from Norway by a deep channel, extending from the Christiania Fjord round the coast northward. Even then this would be everywhere more than another 600 feet deep, and almost as wide as the Strait of Dover.⁵ The ice must cross this and afterwards be forced for more than 300 miles up a slope, which, though gentle, would be in vertical height at least 600 feet. The task, if accomplished by

¹ The present temperature in Ireland over the zone (from S. of Belfast to N. of Galway Bay) which is supposed to have formed the divide of the central snowfield may be given as from 41° to 50° , nearly the same as at the sea-level in Carnarvonshire. Thus, though the district is less mountainous than Wales, it would not need a greater reduction, for the snowfall would probably be rather larger. But this reduction could hardly be less than 20° , for the glaciers would have to form nearly at the

² It is 44.42° at Bergen, 38.48° at Bodo, 35.42° at Hammerfest, 41.36° at Christiania and Stockholm.

³ For particulars see *Geol. Mag.*, 1899, p. 97 (W. H. Hudleston) and p. 282 (T. G. Bonney).

⁴ Christiania and Cape Farewell (Greenland) are nearly on the same latitude.

⁵ For details see *Geol. Mag.*, 1899, pp. 97 and 282.

thrust from behind, would be a heavy one, and, so far as I know, without a parallel at the present day; if the viscosity of the ice enabled it to flow, as has lately been urged,¹ we must be cautious in appealing to the great Antarctic barrier, because we now learn that more than half of it is only consolidated snow.² Moreover, if the ice floated across that channel, the thickness of the boulder-bearing layers would be diminished by melting (as in Ross's Barrier), and the more viscous the material, the greater the tendency for these to be left behind by the overflow of the cleaner upper layers. If, however, the whole region became dry land, the Scandinavian glaciers would descend into a broad valley, considerably more than 1,200 feet deep, which would afford them an easy path to the Arctic Ocean, so that only a lateral overflow, inconsiderable in volume, could spread itself over the western plateau.³ An attempt to escape this difficulty has been made by assuming the existence of an independent centre of distribution for ice and boulders near the middle of the North Sea bed* (which would demand rather exceptional conditions of temperature and precipitation); but in such case either the Scandinavian ice would be fended off from England, or the boulders, prior to its advance, must have been dropped by floating ice on the neighbouring sea-floor.

If, then, our own country were but little better than Spitzbergen as a producer of ice, and Scandinavia only surpassed Southern Greenland in having a rather heavier snowfall, what interpretation may we give to the glacial phenomena of Britain? Three have been proposed. One asserts that throughout the Glacial Epoch the British Isles generally stood at a higher level, so that the ice which almost buried them flowed out on to the beds of the North and Irish Seas. The boulder clays represent its moraines. The stratified sands and gravels were deposited in lakes formed by the rivers which were dammed up by ice-sheets.⁴ A second interpretation recognises the presence of glaciers in the mountain regions, but maintains that the land, at the outset rather above its present level, gradually sank beneath the sea, till the depth of water over the eastern coast of England was fully 500 feet, and

¹ H. M. Decley, *Geol. Mag.*, 1909, p. 239.

² E. Shackleton, *The Heart of the Antarctic*, ii. 277.

³ It has indeed been affirmed (Brögger, *Om de sen-glaciale og post-glaciale nivaeforandringer i Kristianiafjeldet*, p. 682) that at the time of the great ice-sheet of Europe the sea-bottom must have been uplifted at least 8,500 feet higher than at present. This may be a ready explanation of the occurrence of certain dead shells in deep water, but, unless extremely local, it would revolutionise the drainage system of Central Europe.

⁴ *Geol. Mag.*, 1901, pp. 142, 187, 284, 332.

⁵ See Warren Upham, *Monogr. U.S. Geol. Survey*, xxv. (1896). This explanation commends itself to the majority of British geologists as an explanation of the noted parallel roads of Glenroy, but it is peculiar to speak of it as 'conclusively shown' (*Quart. Journ. Geol. Soc.*, lviii. (1902) 473) until a fundamental difficulty which it presents has been discussed and removed.

over the western nearly 1,400 feet, from which depression it slowly recovered. By any such submergence Great Britain and Ireland would be broken up into a cluster of hilly islands, between which the tide from an extended Atlantic would sweep eastwards twice a day, its currents running strong through the narrower sounds, while movements in the reverse direction at the ebb would be much less vigorous. The third interpretation, in some respects intermediate, was first advanced by the late Professor Carvill Lewis, who held that the peculiar boulder clays and associated sands (such as those of East Anglia), which, as was then thought, were not found more than about 450 feet above the present sea-level, had been deposited in a great fresh-water lake, held up by the ice-sheets already mentioned and by an isthmus, which at that time occupied the place of the Strait of Dover. Thus, these deposits, though indirectly due to land-ice, were actually fluvial or lacustrine. But this interpretation need not detain us, though the former existence of such lakes is still maintained, on a small scale in Britain, on a much larger one in North America, because, as was pointed out when it was first advanced, it fails to explain the numerous erratic blocks and shell-bearing sands which occur far above the margin of the hypothetical lake.

Each of the other two hypotheses involves grave difficulties. That of great confluent ice-sheets creeping over the British lowlands demands, as has been intimated, climatal conditions which are scarcely possible, and makes it hard to explain the sands and gravels, sometimes with regular alternate bedding, but more generally indicative of strong current action, which occur at various elevations to over 1,300 feet above sea-level, and seem too widespread to have been formed either beneath an ice-sheet or in lakes held up by one; for the latter, if of any size, would speedily check the velocity of influent streams. Also the mixture and crossing of boulders, which we have described, are inexplicable without the most extraordinary oscillations in the size of the contributing glaciers. To suppose that the Scandinavian ice reached to Bedfordshire and Herts and then retired in favour of North British glaciers, or *vice versa*, assumes an amount of variation which, so far as I am aware, is without a parallel elsewhere. So also the mixture of boulders from South Scotland, the Lake District, and North Wales which lie, especially in parts of Staffordshire and Shropshire, as if dropped upon the surface, far exceeds what may reasonably be attributed to variations amplified by lateral spreading of mountain glaciers on reaching a lowland, while the frequent presence of shells in the drifts, dozens of miles away from the present coast, implies a rather improbable scooping up of the sea-bed without much injury to such fragile objects. The ice also must have been curiously inconstant in

its operations. It is supposed in one place to have glided gently over its bed, in another to have gripped and torn out huge masses of rock.¹ Both actions may be possible in a mountain region, but it is very difficult to understand how they could occur in a lowland or plain. Besides this we can only account for some singular aberrations of boulders, such as Shap granite well above Grosmont in Eskdale, or the Scandinavian rhomb-porphyrty above Lockwood,² near Huddersfield, by assuming a flexibility in the lobes of an ice-sheet which it is hard to match at the present time. Again, the boulder clay of the eastern counties is crowded, as we have described, with pebbles of chalk, which generally are not of local origin, but have come from north of the Wash. Whether from the bed of a river or from a sea-beach, they are certainly water-worn. But if preglacial, the supply would be quickly exhausted, so that they would usually be confined to the lower part of the clay. As it is, though perhaps they run larger here, they abound throughout. The so-called moraines near York (supposed to have been left by a glacier retreating up that vale), those in the neighbourhood of Flamborough Head and of Sheringham (regarded as relics of the North Sea ice-sheet) do not, in my opinion, show any important difference in outline from ordinary hills of sands and gravels, and their materials are wholly unlike those of any indubitable moraines that I have either seen or studied in photographs. It may be said that the British glaciers passed over very different rocks from the Alpine; but the Swiss molasse ought to have supplied abundant sand, and the older interglacial gravels quantities of pebbles; yet the differences between the morainic materials on the flank of the Jura or near the town of Geneva and those close to the foot of the Alps are varietal rather than specific.

Some authorities, however, attribute such magnitude to the ice-sheets radiating from Scandinavia that they depict them, at the time of maximum extension, as not only traversing the North Sea bed and trespassing upon the coast of England, but also radiating southward to overwhelm Denmark and Holland, to invade Northern Germany and Poland, to obliterate Hanover, Berlin, and Warsaw, and to stop but little short of Dresden and Cracow, while burying Russia on the east to within no great distance of the Volga and on the south to the neighbourhood of Kief. Their presence, however, so far as I can ascertain, is inferred from evidence³ very similar to that which we have discussed in the

¹ That this has occurred at Cromer is a very dubious hypothesis (see *Geol. Mag.*, 1905, pp. 397, 524). The curious relations of the drift and chalk in the islands of Mœen and Rügen are sometimes supposed to prove the same action. Knowing both well, I have no hesitation in saying that the chalk there is, as a rule, as much *in situ* as it is in the Isle of Wight.

² About half-way across England and 810 feet above sea-level. P. F. Kendall, *Quart. Journ. Geol. Soc.*, lviii. (1902), p. 498.

³ A valuable summary of it is given in *The Great Ice Age*, J. Geikie, ch. xxix., xxx. (1894).

British lowlands. That Scandinavia was at one time almost wholly buried beneath snow and ice is indubitable; it is equally so that at the outset the land stood above its present level, and that during the later stages of the Glacial Epoch parts, at any rate of Southern Norway, had sunk down to a maximum depth of 800 feet. In Germany, however, erratics are scattered over its plain and stranded on the slopes of the Harz and Riesengebirge up to about 1,400 feet above sea-level. The glacial drifts of the lowlands sometimes contain dislodged masses of neighbouring rocks like those at Cromer, and we read of other indications of ice action. I must, however, observe that since the glacial deposits of Möen, Warnemünde, and Rügen often present not only close resemblances to those of our eastern counties but also very similar difficulties, it is not permissible to quote the one in support of the other, seeing that the origin of each is equally dubious. Given a sufficient 'head' of ice in northern regions, it might be possible to transfer the remains of organisms from the bed of the Irish Sea to Moel Tryfaen, Macclesfield, and Gloppa; but at the last-named, if not at the others, we must assume the existence of steadily alternating currents in the lakes in order to explain the corresponding bedding of the deposit. This, however, is not the only difficulty. The 'Irish Sea glacier' is supposed to have been composed of streams from Ireland, South-West Scotland, and the Lake District, of which the second furnished the dominant contingent; the first-named not producing any direct effect on the western coast of Great Britain, and the third being made to feel its inferiority and 'shouldered in upon the mainland.' But even if this ever happened, ought not the Welsh ice to have joined issue with the invaders a good many miles to the north of its own coast?¹ Welsh boulders at any rate are common near the summit of Moel Tryfaen, and I have no hesitation in saying that the pebbles of riebeckite-rock, far from rare in its drifts, come from Mynydd Mawr, hardly half a league to the E.S.E., and not from Ailsa Craig.²

As such frequent appeal is made to the superior volume of the ice-sheet which poured from the Northern Hills over the bed of the Irish Sea, I will compare in more detail the ice-producing capacities of the

¹ From Moel Tryfaen to the nearest point of Scotland is well over a hundred miles, and it is a few less than this distance from Gloppa to the Lake District. In order to allow the Irish Sea ice-sheet to reach the top of Moel Tryfaen the glacier productive power of Snowdonia has been minimised (Wright, *Man and the Glacial Epoch*, pp. 171, 172). But the difference between that and the Arenig region is not great enough to make the one incompetent to protect its own borderland while the other could send an ice-sheet which could almost cover the Clent Hills and reach the neighbourhood of Birmingham. Anglesey also, if we suppose a slight elevation and a temperature of 29° at the sea-level, would become a centre of ice-distribution and an advance guard to North Wales.

The boulders of pellite near Porth Nobla, from Llanerchymedd, though they have travelled southward, have moved away much to the west.

several districts. The present temperature of West-Central Scotland may be taken as 47° ; its surface as averaging about 2,500 feet, rising occasionally to nearly 4,000 feet above sea-level. In the western part of the Southern Uplands the temperature is a degree higher, and the average for altitude at most not above 1,500 feet. In the Lake District and the Northern Pennines the temperature is increased by another degree, and the heights are, for the one 1,800 feet with a maximum of 3,162 feet, for the other 1,200 feet and 2,892 feet. In North Wales the temperature is 50° , the average height perhaps 2,000 feet, and the culminating point 3,571 feet. For the purpose of comparing the ice-producing powers of these districts we may bring them to one temperature by adding 300 feet to the height for each degree below that of the Welsh region. This would raise the average elevation of Central and Southern Scotland to 3,400 feet and 2,100 feet respectively; for the Lake District and Northern Pennines to 2,100 feet and 1,500 feet. We may picture to ourselves what this would mean, if the snow-line were at the sea-level in North Wales, by imagining 8,000 feet added to its height and comparing it with the Alps. North Wales would then resemble a part of that chain which had an average height of about 10,000 feet above sea-level, and culminated in a peak of 11,571 feet; the Lake District would hardly differ from it; the Northern Pennines would be like a range of about 9,000 feet, its highest peak being 11,192 feet. Southern Scotland would be much the same in average height as the first and second, and would rise, though rarely, to above 11,000 feet; the average in Central Scotland would be about 11,400 feet, and the maximum about 13,000 feet. Thus, North Wales, the Lake District, and the Southern Uplands would differ little in ice-productive power; while Central Scotland would distinctly exceed them, but not more than the group around the Finsteraarhorn does that giving birth to the Rhone glacier. In one respect, however, all these districts would differ from the Alps—that, at 8,000 feet, the surface, instead of being furrowed with valleys, small and great, would be a gently shelving plateau, which would favour the formation of piedmont glaciers. Still, unless we assume the present distribution of rainfall to be completely altered (for which I do not know any reason), the relative magnitudes of the ice coming from these centres (whether separate glaciers or confluent sheets) could differ but little. Scotch ice would not appreciably ‘shoulder inland’ that from the Lake District, nor would the Welsh ice be imprisoned within its own valleys.

During the last few years, however, the lake-hypothesis of Carvill Lewis has been revived under a rather different form by some English advocates of land-ice. For instance, the former presence of ice-dammed lakes is supposed to be indicated in the upper parts of the Cleveland Hills by certain overflow channels. I may be allowed to

observe that, though this view is the outcome of much acute observation and reasoning,¹ it is wholly dependent upon the ice-barriers already mentioned, and that if they dissolve before the dry light of sceptical criticism, the lakes will 'leave not a rack behind.' I must also confess that to my eyes the so-called 'overflow channels' much more closely resemble the remnants of ancient valley-systems, formed by only moderately rapid rivers, which have been isolated by the trespass of younger and more energetic streams, and they suggest that the main features of this picturesque upland were developed before rather than after the beginning of the Glacial Epoch. I think that even 'Lake Pickering,' though it has become an accepted fact with several geologists of high repute, can be more simply explained as a two-branched 'valley of strike,' formed on the Kimeridge clay, the eastern arm of which was beheaded, even in preglacial times, by the sea.² As to Lake Oxford,³ I must confess myself still more sceptical. Some changes no doubt have occurred in later glacial and postglacial times; valleys have been here raised by deposit, there deepened sometimes by as much as 100 feet; the courses of lowland rivers may occasionally have been altered; but I doubt whether, since those times began, either ice-sheet or lake has ever concealed the site of that University city.

The submergence hypothesis assumes that, at the beginning of the Glacial Epoch, our islands stood rather above their present level, and during it gradually subsided, on the west to a greater extent than on the east, till at last the movement was reversed, and they returned nearly to their former position. During most of this time glaciers came down to the sea from the more mountainous islands, and in winter an ice-foot formed upon the shore. This, on becoming detached, carried away boulders, beach pebbles, and finer detritus. Great quantities of the last also were swept by swollen streams, into the estuaries and spread over the sea-bed by coast currents, settling down especially in the quiet depths of submerged valleys. Shore-ice in Arctic regions, as Colonel H. W. Feilden⁴ has described, can striate stones and even the rock beneath it, and is able, on a subsiding area, gradually to push boulders up to a higher level. In fact the state of the British region in those ages would not have been unlike that still existing near the coasts of the Barents and Kara Seas. Over the submerged region southward, and in some cases more or less eastward, currents would

¹ P. F. Kendall, *Quart. Journ. Geol. Soc.*, lviii. (1902), 471.

² See for instance the courses of the Medway and the Beult over the Weald clay (C. Le Neve Foster and W. Topley, *Quart. Journ. Geol. Soc.*, xxi. (1865), p. 443).

³ F. W. Harmer, *Quart. Journ. Geol. Soc.*, lxiii. (1907), p. 470.

⁴ *Quart. Journ. Geol. Soc.*, xxxiv. (1878), p. 556.

be prevalent; though changes of wind¹ would often affect the drift of the floating ice-rafts. But though the submergence hypothesis is obviously free from the serious difficulties which have been indicated in discussing the other one, gives a simple explanation of the presence of marine organisms, and accords with what can be proved to have occurred in Norway, Waigatz Island, Novaia Zemlya, on the Lower St. Lawrence, in Grinnell Land, and elsewhere,² it undoubtedly involves others. One of them—the absence of shore terraces, caves, or other sea marks—is perhaps hardly so grave as it is often thought to be. It may be met by the remark that unless the Glacial Age lasted for a very long time and the movements were interrupted by well-marked pauses, we could not expect to find any such record. In regard also to another objection, the rather rare and sporadic occurrence of marine shells, the answer would be that, on the Norway coast, where the ice-worn rock has certainly been submerged, sea-shells are far from common and occur sporadically in the raised deltaic deposits of the fjords.³ An advocate of this view might also complain, not without justice, that, if he cited an inland terrace, it was promptly dismissed as the product of an ice-dammed lake, and his frequent instances of marine shells in stratified drifts were declared to have been transported from the sea by the lobe of an ice-sheet; even if they have been carried across the path of the Arenig ice, more than forty miles, as the crow flies, from the Irish Sea up the Valley of the Severn, or forced some 1,300 feet up Moel Tryfaen.⁴ The difficulty in the latter case, he would observe, is not met by saying the ice-sheet would be able to climb that hill 'given there were a sufficient head behind it.'⁵ That ice can be driven uphill has long been known, but the existence of the 'sufficient head' must be demonstrated, not assumed. There may be 'no logical halting-place between an uplift of ten or twenty feet to surmount a *roche moutonnée* and an equally gradual

¹ See p. 23, and for the currents now dominant consult Dr. H. Bassett in Professor Herdman's Report on the Lancashire Sea Fisheries, *Trans. Biol. Soc. Liverpool*, xxiv. (1910), p. 123.

² See *Ice Work*, p. 221, and *Geol. Mag.*, 1900, p. 289.

³ If, as seems probable, the temperature was changing rather rapidly the old fauna would be pauperised and the new one make its way but slowly into the British fjords.

⁴ Critics of the submergence hypothesis seem to find a difficulty in admitting downward and upward movements amounting sometimes to nearly 1,400 feet during Pleistocene Ages; but in the northern part of America the upheaval, at any rate, has amounted to about 1,000 feet, while on the western coast, beneath the lofty summit of Mount St. Elias, marine shells of existing species have been obtained some 5,000 feet above sea-level. It is also admitted that in several places the pre-glacial surface of the land was much above its present level. On the Red River, whatever be the explanation, foraminifera, radiolarians, and sponge spicules have been found at 700 feet above sea-level, and near Victoria, on the Saskatchewan, even up to about 1,900 feet.

⁵ P. F. Kendall in Wright's *Man and the Glacial Period*, p. 171.

elevation to the height of Moel Tryfaen,' yet there is a common-sense limitation, even to a destructive *sortes*. The argument, in fact, is more specious than valid, till we are told approximately how thick the northern ice must be to produce the requisite pressure, and whether such an accumulation would be possible. The advocates of land ice admit that, before it had covered more than a few leagues on its southward journey its thickness was less than 2,000 feet, and we are not entitled, as I have endeavoured to show, to pile up ice indefinitely on either our British highlands or the adjacent sea-bed. The same reason also forbids us largely to augment the thickness of the latter by the snowfall on its surface, as happens to the Antarctic barrier ice. Even if the thickness of the ice-cap over the Dumfries and Kirkcudbright hills had been about 2,500 feet, that, with every allowance for viscosity, would hardly give us a head sufficient to force a layer of ice from the level of the sea-bed to a height of nearly 1,400 feet above it and at a distance of more than 100 miles.

Neither can we obtain much support from the instance in Spitzbergen, described by Professors Garwood and Gregory, where the Ivory Glacier, after crossing the bed of a valley, had transported marine shells and drift from the floor (little above sea-level) to a height of about 400 feet on the opposite slope. Here the valley was narrow, and the glacier had descended from an inland ice-reservoir, much of which was at least 2,800 feet above the sea, and rose occasionally more than a thousand feet higher.¹

But other difficulties are far more grave. The thickness of the chalky boulder clay alone, as has been stated, not unfrequently exceeds 100 feet, and, though often much less, may have been reduced by denudation. This is an enormous amount to have been transported and distributed by floating ice. The materials also are not much more easily accounted for by this than by the other hypothesis. A continuous supply of well-worn chalk pebbles might indeed be kept up from a gradually rising or sinking beach, but it is difficult to see how, until the land had subsided for at least 200 feet, the chalky boulder clay could be deposited in some of the East Anglian valleys or on the Leicestershire hills. That depression, however, would seriously diminish the area of exposed chalk in Lincolnshire and Yorkshire, and the double of it would almost drown that rock. Again, the East Anglian boulder clay, as we have said, frequently abounds in fragments and finer detritus from the Kimeridge and Oxford clays. But a large part of their outcrop would disappear before the former submergence was completed. Yet the materials of the boulder clay, though changing as it is traced across the country, more especially from east to west, seem to vary little in a

¹ *Quart. Jour. Geol. Soc.*, liv. (1898), p. 205. Earlier observations of some upthrust of materials by a glacier are noted on p. 219.

vertical direction. The instances, also, of the transportation of boulders and smaller stones to higher levels, sometimes large in amount, as in the transference of 'brockram' from outcrops near the bed of the Eden valley to the level of Stainmoor Gap, seem to be too numerous to be readily explained by the uplifting action of shore-ice in a subsiding area. Such a process is possible, but we should anticipate it would be rather exceptional.

Submergence also readily accounts for the above-named sands and gravels, but not quite so easily for their occurrence at such very different levels. On the eastern side of England gravelly sands may be found beneath the chalky boulder clay from well below sea-level to three or four hundred feet above it. Again, since, on the submergence hypothesis, the lower boulder clay about the estuaries of the Dee and the Mersey must represent a deposit from piedmont ice in a shallow sea, the mid-glacial sand (sometimes not very clearly marked in this part) ought not to be more than forty or fifty feet above the present Ordnance datum. But at Manchester it reaches over 200 feet, while near Heywood it is at least 425 feet. In other words the sands and gravels, presumably (often certainly) mid-glacial, mantle, like the upper boulder clay, over great irregularities of the surface, and are sometimes found, as already stated, up to more than 1,200 feet. Either of these deposits may have followed the sea-line upwards or downwards, but that explanation would almost compel us to suppose that the sand was deposited during the submergence and the upper clay during the emergence; so that, with the former material, the higher in position is the newer in time, and with the latter the reverse. We must not, however, forget that in the island of Rügen we find more than one example of a stratified gravelly sand between two beds of boulder clay (containing Scandinavian erratics) which present some resemblance to the boulder clays of Eastern England, while certain glacial deposits at Warnemünde, on the Baltic coast, sometimes remind us of the Contorted Drift of Norfolk.

Towards the close of the Glacial Epoch, the deposition of the boulder clay ceased¹ and its denudation began. On the low plateaux of the Eastern Counties it is often succeeded by coarse gravels, largely composed of flint, more or less water-worn. These occasionally include small intercalations of boulder clay, have evidently been derived from it, and indicate movement by fairly strong currents. Similar gravels are found overlying the boulder clay in other parts of England, sometimes at greater heights above sea-level. Occasionally the two are intimately related. For instance, a pit on the broad, almost level, top of the Gogmagog Hills, about 200 feet above sea-level and four miles south of Cambridge, shows a current-bedded sand and gravel, overlain by a

¹ Probably deposits of a distinctly glacial origin (such as those near Hesse in Yorkshire) continued in the northern districts, but on these we need not linger.

boulder clay, obviously rearranged; while other pits in the immediate neighbourhood expose varieties and mixtures of one or the other material. But, as true boulder clay occurs in the valley below, these gravels must have been deposited, and that by rather strong currents, on a hill-top—a thing which seems impossible under anything like the existing conditions; and, even if the lowland were buried beneath ice full 200 feet in thickness, which made the hill-top into the bed of a lake, it is difficult to understand how the waters of that could be in rapid motion. Rearranged boulder clays also occur on the slopes of valleys¹ which may be explained, with perhaps some of the curious sections near Sudbury, by the slipping of materials from a higher position. But at Old Oswestry gravels with indications of ice action are found at the foot of the hills almost 700 feet below those of Gloppa.

Often the plateau gravels are followed at a lower level by terrace gravels,² which descend towards the existing rivers, and suggest that valleys have been sometimes deepened, sometimes only re-excavated. The latter gravels are obviously deposited by rivers larger and stronger than those which now wind their way seawards, but it is difficult to explain the former gravels by any fluvial action, whether the water from a melting ice-sheet ran over the land or into a lake, held up by some temporary barrier. But the sorting action of currents in a slowly shallowing sea would be quite competent to account for them, so they afford an indirect support to the hypothesis of submergence. It is, however, generally admitted that there have been oscillations both of level and of climate since any boulder clay was deposited in the districts south of the Humber and the Ribble. The passing of the Great Ice Age was not sudden, and glaciers may have lingered in our mountain regions when palæolithic man hunted the mammoth in the valley of the Thames, or frequented the caves of Devon and Mendip. But of these times of transition before written history became possible, and of sundry interesting topics connected with the Ice Age itself—of its cause, date, and duration, whether it was persistent or interrupted by warmer episodes, and, if so, by what number, of how often it had already recurred in the history of the earth—I must, for obvious reasons, refrain from speaking, and content myself with having endeavoured to place before you the facts of which, in my opinion, we must take account in reconstructing the physical geography of Western Europe, and especially of our own country, during the Age of Ice.

Not unnaturally you will expect a decision in favour of one or the other litigant after this long summing up. But I can only say that, in regard to the British Isles, the difficulties in either hypothesis appear so

¹ For instance, at Stanningfield in the valley of the Lark.

² These contain the instruments worked by palæolithic (Acheulean) man who, in this country at any rate, is later than the chalky boulder clay.

great that, while I consider those in the 'land-ice' hypothesis to be the more serious, I cannot as yet declare the other one to be satisfactorily established, and think we shall be wiser in working on in the hope of clearing up some of the perplexities. I may add that, for these purposes, regions like the northern coasts of Russia and Siberia appear to me more promising than those in closer proximity to the North or South Magnetic Poles. This may seem a 'lame and impotent conclusion' to so long a disquisition, but there are stages in the development of a scientific idea when the best service we can do it is by attempting to separate facts from fancies, by demanding that difficulties should be frankly faced instead of being severely ignored, by insisting that the giving of a name cannot convert the imaginary into the real, and by remembering that if hypotheses yet on their trial are treated as axioms, the result will often bring disaster, like building a tower on a foundation of sand. To scrutinise, rather than to advocate any hypothesis, has been my aim throughout this address, and, if my efforts have been to some extent successful, I trust to be forgiven, though I may have trespassed on your patience and disappointed a legitimate expectation.

British Association for the Advancement of Science.

PORTSMOUTH, 1911.

ADDRESS

BY

PROFESSOR SIR WILLIAM RAMSAY, K.C.B., PH.D., LL.D.,
D.Sc., M.D., F.R.S.,

PRESIDENT.

It is now eighty years since this Association first met at York, under the presidency of Earl Fitzwilliam. The object of the Association was then explicitly stated:—‘To give a stronger impulse and a more systematic direction to scientific inquiry, to promote the intercourse of those who cultivate science in different parts of the British Empire with one another and with foreign philosophers, to obtain a more general attention to the objects of science and a removal of any disadvantages of a public kind which impede its progress.’

In 1831 the workers in the domain of science were relatively few. The Royal Society, which was founded by Dr. Willis, Dr. Wilkins, and others, under the name of the ‘Invisible, or Philosophical College,’ about the year 1645, and which was incorporated in December 1660, with the approval of King Charles II, was almost the only meeting-place for those interested in the progress of science; and its Philosophical Transactions, begun in March 1664-5, almost the only medium of publication. Its character was described in the following words of a contemporary poem:—

‘This noble learned Corporation
Not for themselves are thus combined
To prove all things by demonstration
But for the public good of the nation,
And general benefit of mankind.’

The first to hive off from the Royal Society was the Linnean Society for the promotion of botanical studies, founded in 1788 by Sir James Edward Smith, Sir Joseph Banks, and other Fellows of the Royal Society; in 1807 it was followed by the Geological Society; at a later date the Society of Antiquaries, the Chemical, the Zoological, the Physical, the Mathematical, and many other Societies were founded. And it was felt by those capable of forming a judgment that, as well

expressed by Lord Playfair at Aberdeen in 1885, 'Human progress is so identified with scientific thought, both in its conception and realisation, that it seems as if they were alternative terms in the history of civilisation.' This is only an echo through the ages of an utterance of the great Englishman, Roger Bacon, who wrote in 1250 A.D.: 'Experimental science has three great prerogatives over all other sciences: it verifies conclusions by direct experiment; it discovers truths which they could never reach; and it investigates the secrets of Nature, and opens to us a knowledge of the past and of the future.'

The world has greatly changed since 1831; the spread of railways and the equipment of numerous lines of steamships have contributed to the peopling of countries at that time practically uninhabited. Moreover, not merely has travelling been made almost infinitely easier, but communication by post has been enormously expedited and cheapened; and the telegraph, the telephone, and wireless telegraphy have simplified as well as complicated human existence. Furthermore, the art of engineering has made such strides that the question 'Can it be done?' hardly arises, but rather 'Will it pay to do it?' In a word, the human race has been familiarised with the applications of science; and men are ready to believe almost anything, if brought forward in its name.

Education, too, in the rudiments of science has been introduced into almost all schools; young children are taught the elements of physics and chemistry. The institution of a Section for Education in our Association (L) has had for its object the organising of such instruction, and much useful advice has been proffered. 'The problem is, indeed, largely an educational one; it is being solved abroad in various ways—in Germany and in most European States by elaborate Governmental schemes dealing with elementary and advanced instruction, literary, scientific, and technical; and in the United States and in Canada by the far-sightedness of the people: both employers and employees recognise the value of training and of originality, and on both sides sacrifices are made to ensure efficiency.

In England we have made technical education a local, not an Imperial question; instead of half a dozen first-rate institutions of University rank, we have a hundred, in which the institutions are necessarily understaffed, in which the staffs are mostly overworked and underpaid; and the training given is that not for captains of industry, but for workmen and foremen. 'Efficient captains cannot be replaced by a large number of fairly good corporals.' Moreover, to induce scholars to enter these institutions, they are bribed by scholarships, a form of pauperisation practically unknown in every country but our own; and to crown the edifice, we test results by examinations of a kind not adapted to gauge originality and character (if, indeed, these can

ever be tested by examination), instead of, as on the Continent and in America, trusting the teachers to form an honest estimate of the capacity and ability of each student, and awarding honours accordingly.

The remedy lies in our own hands. Let me suggest that we exact from all gainers of University scholarships an undertaking that, if and when circumstances permit, they will repay the sum which they have received as a scholarship, bursary, or fellowship. It would then be possible for an insurance company to advance a sum representing the capital value, viz. 7,464,931*l.*, of the scholarships, reserving, say, twenty per cent. for non-payment, the result of mishap or death. In this way a sum of over six million pounds, of which the interest is now expended on scholarships, would be available for University purposes. This is about one-fourth of the sum of twenty-four millions stated by Sir Norman Lockyer at the Southport meeting as necessary to place our University education on a satisfactory basis. A large part of the income of this sum should be spent in increasing the emoluments of the chairs; for, unless the income of a professor is made in some degree commensurate with the earnings of a professional man who has succeeded in his profession, it is idle to suppose that the best brains will be attracted to the teaching profession. And it follows that unless the teachers occupy the first rank, the pupils will not be stimulated as they ought to be.

Again, having made the profession of a teacher so lucrative as to tempt the best intellects in the country to enter it, it is clear that such men are alone capable of testing their pupils. The modern system of 'external examinations,' known only in this country, and answerable for much of its lethargy, would disappear; schools of thought would arise in all subjects, and the intellectual as well as the industrial prosperity of our nation would be assured. As things are, can we wonder that as a nation we are not scientific? Let me recommend those of my hearers who are interested in the matter to read a recent report on Technical Education by the Science Guild.

I venture to think that, in spite of the remarkable progress of science and of its applications, there never was a time when missionary effort was more needed. Although most people have some knowledge of the results of scientific inquiry, few, very few, have entered into its spirit. We all live in hope that the world will grow better as the years roll on. Are we taking steps to secure the improvement of the race? I plead for recognition of the fact that progress in science does not only consist in accumulating information which may be put to practical use, but in developing a spirit of prevision, in taking thought for the morrow; in attempting to forecast the future, not by vague surmise, but by orderly marshalling of facts, and by deducing from them their logical outcome; and chiefly in endeavouring to control conditions which may

be utilised for the lasting good of our people. We must cultivate a belief in the 'application of trained intelligence to all forms of national activity.'

The Council of the Association has had under consideration the formation of a Section of Agriculture. For some years this important branch of applied science, borrowing as it does from botany, from physics, from chemistry, and from economics, has in turn enjoyed the hospitality of each of these sections, itself having been made a sub-section of one of these more definite sciences. It is proposed this year to form an Agricultural Section. Here, there is need of missionary effort; for our visits to our colonies have convinced many of us that much more is being done for the farmer in the newer parts of the British Empire than at home. Agriculture is, indeed, applied botany, chemistry, entomology, and economics; and has as much right to independent treatment as has engineering, which may be strictly regarded as applied physics.

The question has often been debated whether the present method of conducting our proceedings is the one best adapted to gain our ends. We exist professedly 'to give a stronger impulse and a more systematic direction to scientific inquiry.' The Council has had under consideration various plans framed with the object of facilitating our work, and the result of its deliberations will be brought under your attention at a later date. To my mind, the greatest benefit bestowed on science by our meetings is the opportunity which they offer for friendly and unrestrained intercourse, not merely between those following different branches of science, but also with persons who, though not following science professionally, are interested in its problems. Our meetings also afford an opportunity for younger men to make the acquaintance of older men. I am afraid that we who are no longer in the spring of our lifetime, perhaps from modesty, perhaps through carelessness, often do not sufficiently realise how stimulating to a young worker a little sympathy can be; a few words of encouragement go a long way. I have in my mind words which encouraged me as a young man, words spoken by the leaders of Associations now long past—by Playfair, by Williamson, by Frankland, by Kelvin, by Stokes, by Francis Galton, by Fitzgerald, and many others. Let me suggest to my older scientific colleagues that they should not let such pleasant opportunities slip.

Since our last meeting the Association has to mourn the loss by death of many distinguished members. Among these are:—

Dr. John Beddoe, who served on the Council from 1870 to 1875, has recently died at a ripe old age, after having achieved a world-wide reputation by his magnificent work in the domain of anthropology.

Sir Rubert Boyce, called away at a comparatively early age in the middle of his work, was for long a colleague of mine at University

College, and was one of the staff of the Royal Commission on Sewage Disposal. The service he rendered science in combating tropical diseases is well known.

Sir Francis Galton died at the beginning of the year, at the advanced age of 89. His influence on science has been characterised by Professor Karl Pearson in his having maintained the idea that exact quantitative methods could—nay, must—be applied to many branches of science which had been held to be beyond the field of either mathematical or physical treatment. Sir Francis was General Secretary of this Association from 1863 to 1868; he was President of Section E in 1862, and again in 1872; he was President of Section H in 1885; but, although often asked to accept the office of President of the Association, his consent could never be obtained. Galton's name will always be associated with that of his friend and relative, Charles Darwin, as one of the most eminent and influential of English men of science.

Professor Thomas Rupert Jones, also, like Galton, a member of this Association since 1860, and in 1891 President of the Geological Section, died in April last at the advanced age of 91. Like Dr. Beddoe, he was a medical man with wide scientific interests. He became a distinguished geologist, and for many years edited the Quarterly Journal of the Geological Society.

Professor Story Maskelyne, at one time a diligent frequenter of our meetings, and a member of the Council from 1874 to 1880, was a celebrated mineralogist and crystallographer. He died at the age of 88. The work which he did in the University of Oxford and at the British Museum is well known. In his later life he entered Parliament.

Dr. Johnstone Stoney, President of Section A in 1897, died on July 1, in his 86th year. He was one of the originators of the modern view of the nature of electricity, having given the name 'electron' to its unit as far back as 1874. His investigations dealt with spectroscopy and allied subjects, and his philosophic mind led him to publish a scheme of ontology which, I venture to think, must be acknowledged to be the most important work which has even been done on that difficult subject.

Among our corresponding members we have lost Professor Bohr, of Copenhagen; Professor Brühl, of Heidelberg; Hofrat Dr. Caro, of Berlin; Professor Fittig, of Strassburg; and Professor Van't Hoff, of Berlin. I cannot omit to mention that veteran of science, Professor Cannizzaro, of Rome, whose work in the middle of last century placed chemical science on the firm basis which it now occupies.

I knew all these men, some of them intimately; and, if I have not ventured on remarks as to their personal qualities, it is because it may be said of all of them that they fought a good fight and maintained

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the faith that only by patient and unceasing scientific work is human progress to be hoped for.

It has been the usual custom of my predecessors in office either to give a summary of the progress of science within the past year or to attempt to present in intelligible language some aspect of the science in which they have themselves been engaged. I possess no qualifications for the former course, and I therefore ask you to bear with me while I devote some minutes to the consideration of ancient and modern views regarding the chemical elements. To many in my audience part of my story will prove an oft-told tale; but I must ask those to excuse me, in order that it may be in some wise complete.

In the days of the early Greeks the word 'element' was applied rather to denote a property of matter than one of its constituents. Thus, when a substance was said to contain fire, air, water, and earth (of which terms a childish game doubtless once played by all of us is a relic), it probably meant that they partook of the nature of the so-called elements. Inflammability showed the presence of concealed fire; the escape of 'airs' when some substances are heated or when vegetable or animal matter is distilled no doubt led to the idea that these airs were imprisoned in the matters from which they escaped; hardness and permanence were ascribed to the presence of earth, while liquidity and fusibility were properties conveyed by the presence of concealed water. At a later date the 'Spagyrics' added three 'hypostatical principles' to the quadrilateral; these were 'salt,' 'sulphur,' and 'mercury.' The first conveyed solubility, and fixedness in fire; the second, inflammability; and the third, the power which some substances manifest of producing a liquid, generally termed 'phlegm,' on application of heat, or of themselves being converted into the liquid state by fusion.

It was Robert Boyle, in his 'Skeptical Chymist,' who first controverted these ancient and medieval notions, and who gave to the word 'element' the meaning that it now possesses—the constituent of a compound. But in the middle of the seventeenth century chemistry had not advanced far enough to make his definition useful; for he was unable to suggest any particular substance as elementary. And, indeed, the main tenet of the doctrine of 'phlogiston,' promulgated by Stahl in the eighteenth century, and widely accepted, was that all bodies capable of burning or of being converted into a 'calx,' or earthly powder, did so in virtue of the escape of a subtle fluid from their pores; this fluid could be restored to the 'calces' by heating them with other substances rich in phlogiston, such as charcoal, oil, flour, and the like. Stahl, however false his theory, had at least the merit of having constructed a reversible chemical equation:—Metal—phlogiston=Calx; Calx+phlogiston=Metal.

It is difficult to say when the first element was known to be an element. After Lavoisier's overthrow of the phlogistic hypothesis, the part played by oxygen, then recently discovered by Priestley and Scheele, came prominently forward. Loss of phlogiston was identified with oxidation; gain of phlogiston, with loss of oxygen. The scheme of nomenclature ('Méthode de Nomenclature chimique'), published by Lavoisier in conjunction with Guyton de Morveau, Berthollet, and Fourcroy, created a system of chemistry out of a wilderness of isolated facts and descriptions. Shortly after, in 1789, Lavoisier published his 'Traité de Chimie,' and in the preface the words occur: 'If we mean by "elements" the simple and indivisible molecules of which bodies consist, it is probable that we do not know them; if, on the other hand, we mean the last term in analysis, then every substance which we have not been able to decompose is for us an element; not that we can be certain that bodies which we regard as simple are not themselves composed of two or even a larger number of elements, but because these elements can never be separated, or rather, because we have no means of separating them, they act, so far as we can judge, as elements; and we cannot call them "simple" until experiment and observation shall have furnished a proof that they are so.'

The close connection between 'crocus of Mars' and metallic iron, the former named by Lavoisier 'oxyde de fer,' and similar relations between metals and their oxides, made it likely that bodies which reacted as oxides in dissolving in acids and forming salts must also possess a metallic substratum. In October 1807, Sir Humphry Davy proved the correctness of this view for soda and potash by his famous experiment of splitting these bodies by a powerful electric current into oxygen and hydrogen on the one hand, and the metals sodium and potassium on the other. Calcium, barium, strontium, and magnesium were added to the list as constituents of the oxides, lime, barytes, strontia, and magnesia. Some years later Scheele's 'dephlogisticated marine acid,' obtained by heating pyrolusite with 'spirit of salt,' was identified by Davy as in all likelihood elementary. His words are: 'All the conclusions which I have ventured to make respecting the undecomposed nature of oxymuriatic gas are, I conceive, entirely confirmed by these new facts.' 'It has been judged most proper to suggest a name founded upon one of its obvious and characteristic properties, its colour, and to call it chlorine.' The subsequent discovery of iodine by Courtois in 1812, and of bromine by Balard in 1826, led to the inevitable conclusion that fluorine, if isolated, should resemble the other halogens in properties, and much later, in the able hands of Moissan, this was shown to be true.

The modern conception of the elements was much strengthened by Dalton's revival of the Greek hypothesis of the atomic constitution of

matter, and the assigning to each atom a definite weight. This momentous step for the progress of chemistry was taken in 1803; the first account of the theory was given to the public with Dalton's consent in the third edition of Thomas Thomson's 'System of Chemistry' in 1807; it was subsequently elaborated in the first volume of Dalton's own 'System of Chemical Philosophy,' published in 1808. The notion that compounds consisted of aggregations of atoms of elements, united in definite or multiple proportions, familiarised the world with the conception of elements as the bricks of which the Universe is built. Yet the more daring spirits of that day were not without hope that the elements themselves might prove decomposable. Davy, indeed, went so far as to write in 1811: 'It is the duty of the chemist to be bold in pursuit; he must recollect how contrary knowledge is to what appears to be experience. . . . To enquire whether the elements be capable of being composed and decomposed is a grand object of true philosophy.' And Faraday, his great pupil and successor, at a later date, 1815, was not behind Davy in his aspirations, when he wrote: 'To decompose the metals, to re-form them, and to realise the once absurd notion of transformation—these are the problems now given to the chemist for solution.'

Indeed, the ancient idea of the unitary nature of matter was in those days held to be highly probable. For attempts were soon made to demonstrate that the atomic weights were themselves multiples of that of one of the elements. At first the suggestion was that oxygen was the common basis; and later, when this supposition turned out to be untenable, the claims of hydrogen were brought forward by Prout. The hypothesis was revived in 1842 when Liebig and Redtenbacher, and subsequently Dumas, carried out a revision of the atomic weights of some of the commoner elements, and showed that Berzelius was in error in attributing to carbon the atomic weight 12.25, instead of 12.00. Of recent years a great advance in the accuracy of the determinations of atomic weights has been made, chiefly owing to the work of Richards and his pupils, of Gray, and of Guye and his collaborators, and every year an international committee publishes a table in which the most probable numbers are given on the basis of the atomic weight of oxygen being taken as sixteen. In the table for 1911, of eighty-one elements no fewer than forty-three have recorded atomic weights within one-tenth of a unit above or below an integral number. My mathematical colleague, Karl Pearson, assures me that the probability against such a condition being fortuitous is 20,000 millions to one.

The relation between the elements has, however, been approached from another point of view. After preliminary suggestions by Döbereiner, Dumas, and others, John Newlands in 1862 and the following years arranged the elements in the numerical order of their atomic

weights, and published in the *Chemical News* of 1863 what he termed his law of octaves—that every eighth element, like the octave of a musical note, is in some measure a repetition of its forerunner. Thus, just as C on the third space is the octave of C below the line, so potassium, in 1863 the eighth known element numerically above sodium, repeats the characters of sodium, not only in its physical properties—colour, softness, ductility, malleability, &c.—but also in the properties of its compounds, which, indeed, resemble each other very closely. The same fundamental notion was reproduced at a later date and independently by Lothar Meyer and Dmitri Mendeléeff; and to accentuate the recurrence of such similar elements in *periods*, the expression ‘the periodic system of arranging the elements’ was applied to Newlands’ arrangement in octaves. As everyone knows, by help of this arrangement Mendeléeff predicted the existence of then unknown elements, under the names of eka-boron, eka-aluminium, and eka-silicon, since named *scandium*, *gallium*, and *germanium*, by their discoverers, Cleve, Lecoq de Boisbaudran, and Winckler.

It might have been supposed that our knowledge of the elements was practically complete; that perhaps a few more might be discovered to fill the outstanding gaps in the periodic table. True, a puzzle existed and still exists in the classification of the ‘rare earths,’ oxides of metals occurring in certain minerals; these metals have atomic weights between 139 and 180, and their properties preclude their arrangement in the columns of the periodic table. Besides these, the discovery of the inert gases of the atmosphere, of the existence of which Johnstone Stoney’s spiral curve, published in 1888, pointed a forecast, joined the elements like sodium and potassium, strongly electro-negative, to those like fluorine and chlorine, highly electro-positive, by a series of bodies electrically as well as chemically inert, and neon, argon, krypton, and xenon formed links between fluorine and sodium, chlorine and potassium, bromine and rubidium, and iodine and caesium.

Including the inactive gases, and adding the more recently discovered elements of the rare earths, and radium, of which I shall have more to say presently, there are eighty-four definite elements, all of which find places in the periodic table, if merely numerical values be considered. Between lanthanum, with atomic weight 139, and tantalum, 181, there are in the periodic table seventeen spaces; and although it is impossible to admit, on account of their properties, that the elements of the rare earths can be distributed in successive columns (for they all resemble lanthanum in properties), yet there are now fourteen such elements; and it is not improbable that other three will be separated from the complex mixture of their oxides by further work. Assuming that the metals of the rare earths fill these seventeen spaces, how many still remain to be filled? We will take for granted that the

atomic weight of uranium, 238.5, which is the highest known, forms an upper limit not likely to be surpassed. It is easy to count the gaps; there are eleven.

But we are confronted by an *embarras de richesse*. The discovery of radioactivity by Henri Becquerel, of radium by the Curies, and the theory of the disintegration of the radioactive elements, which we owe to Rutherford and Soddy, have indicated the existence of no fewer than twenty-six elements hitherto unknown. To what places in the periodic table can they be assigned?

But what proof have we that these substances are elementary? Let us take them in order.

Beginning with radium, its salts were first studied by Madame Curie; they closely resemble those of barium—sulphate, carbonate, and chromate insoluble; chloride and bromide similar in crystalline form to chloride and bromide of barium; metal, recently prepared by Madame Curie, white, attacked by water, and evidently of the type of barium. The atomic weight, too, falls into its place; as determined by Madame Curie and by Thorpe, it is 89.5 units higher than that of barium; in short, there can be no doubt that radium fits the periodic table, with an atomic weight of about 226.5. It is an undoubted element.

But it is a very curious one. For it is *unstable*. Now, stability was believed to be the essential characteristic of an element. Radium, however, disintegrates—that is, changes into other bodies, and at a constant rate. If 1 gram of radium is kept for 1760 years, only half a gram will be left at the end of that time; half of it will have given other products. What are they? We can answer that question. Rutherford and Soddy found that it gives a condensable gas, which they named 'radium-emanation'; and Soddy and I, in 1903, discovered that, in addition, it evolves helium, one of the inactive series of gases, like argon. Helium is an undoubted element, with a well-defined spectrum; it belongs to a well-defined series. And radium-emanation, which was shown by Rutherford and Soddy to be incapable of chemical union, has been liquefied and solidified in the laboratory of University College, London; its spectrum has been measured and its density determined. From the density the atomic weight can be calculated, and it corresponds with that of a congener of argon, the whole series being: helium, 4; neon, 20; argon, 40; krypton, 83; xenon, 130; unknown, about 178; and niton (the name proposed for the emanation to recall its connection with its congeners, and its phosphorescent properties), about 222.4. The formation of niton from radium would therefore be represented by the equation: radium (226.4) = helium (4) + niton (222.4).

Niton, in its turn, disintegrates, or decomposes, and at a rate much more rapid than the rate of radium; half of it has changed in about four

days. Its investigation, therefore, had to be carried out very rapidly, in order that its decomposition might not be appreciable while its properties were being determined. Its product of change was named by Rutherford 'radium A,' and it is undoubtedly deposited from niton as a metal, with simultaneous evolution of helium; the equation would therefore be:—niton (222.4) = helium (4) + radium A (218.4). But it is impossible to investigate radium A chemically, for in three minutes it has half changed into another solid substance, radium B, again giving off helium. This change would be represented by the equation:—radium A (218.4) = helium (4) + radium B (214.4). Radium B, again, can hardly be examined chemically, for in twenty-seven minutes it has half changed into radium C¹. In this case, however, no helium is evolved; only atoms of negative electricity, to which the name 'electrons' has been given by Dr. Stoney, and these have minute weight which, although approximately ascertainable, at present has defied direct measurement. Radium C¹ has a half-life of 19.5 minutes; too short, again, for chemical investigation; but it changes into radium C², and in doing so, each atom parts with a helium atom; hence the equation:—radium C¹ (214.4) = helium (4) + radium C² (210.4). In 2.5 minutes, radium C² is half gone, parting with electrons, forming radium D. Radium D gives the chemist a chance, for its half-life is no less than sixteen and a half years. Without parting with anything detectable, radium D passes into radium E, of which the half-life period is five days; and lastly radium E changes spontaneously into radium 'F', the substance to which Madame Curie gave the name 'polonium' in allusion to her native country, Poland. Polonium, in its turn, is half changed in 140 days with loss of an atom of helium into an unknown metal, supposed to be possibly lead. If that be the case, the equation would run:—polonium (210.4) = helium (4) + lead (206.4). But the atomic weight of lead is 207.1, and not 206.4; however, it is possible that the atomic weight of radium is 227.1, and not 226.4.

We have another method of approaching the same subject. It is practically certain that the progenitor of radium is uranium; and that the transformation of uranium into radium involves the loss of three alpha particles; that is, of three atoms of helium. The atomic weight of helium may be taken as one of the most certain; it is 3.994, as determined by Mr. Watson, in my laboratories. Three atoms would therefore weigh 11.98, practically 12. There is, however, still some uncertainty in the atomic weight of uranium; Richards and Merigold make it 239.4; but the general mean, calculated by Clarke, is 239.0. Subtracting 12 from these numbers, we have the values 227.0, and 227.4 for the atomic weight of radium. It is as yet impossible to draw any certain conclusion.

The importance of the work which will enable a definite and sure

conclusion to be drawn is this:—For the first time, we have accurate knowledge as to the descent of some of the elements. Supposing the atomic weight of uranium to be certainly 239, it may be taken as proved that in losing three atoms of helium, radium is produced, and, if the change consists solely in the loss of the three atoms of helium, the atomic weight of radium must necessarily be 227. But it is known that β -rays, or electrons, are also parted with during this change; and electrons have weight. How many electrons are lost is unknown; therefore, although the weight of an electron is approximately known, it is impossible to say how much to allow for in estimating the atomic weight of radium. But it is possible to solve this question indirectly, by determining exactly the atomic weights of radium and of uranium; the difference between the atomic weight of radium *plus* 12, *i.e.*, plus the weight of three atoms of helium, and that of uranium, will give the weight of the number of electrons which escape. Taking the most probable numbers available, *viz.*, 239.4 for uranium, and 226.8 for radium, and adding 12 to the latter, the weight of the escaping electrons would be 0.6.

The correct solution of this problem would in great measure clear up the mystery of the irregularities in the periodic table, and would account for the deviations from Prout's Law, that the atomic weights are multiples of some common factor or factors. I also venture to suggest that it would throw light on allotropy, which in some cases at least may very well be due to the loss or gain of electrons, accompanied by a positive or negative heat-change. Incidentally, this suggestion would afford places in the periodic table for the somewhat overwhelming number of pseudo-elements the existence of which is made practically certain by the disintegration hypothesis. Of the twenty-six elements derived from uranium, thorium, and actinium, ten, which are formed by the emission of electrons alone, may be regarded as allotropes or pseudo-elements; this leaves sixteen, for which sixteen or seventeen gaps would appear to be available in the periodic table, provided the reasonable supposition be made that a second change in the length of the periods has taken place. It is above all things certain that it would be a fatal mistake to regard the existence of such elements as irreconcilable with the periodic arrangement, which has rendered to systematic chemistry such signal service in the past.

Attention has repeatedly been drawn to the enormous quantity of energy stored up in radium and its descendants. That in its emanation, niton, is such that if what it parts with as heat during its disintegration were available, it would be equal to three and a half million times the energy available by the explosion of an equal volume of detonating gas—a mixture of one volume of oxygen with two volumes of hydrogen.

The major part of this energy comes, apparently, from the expulsion of particles (that is, of atoms of helium) with enormous velocity. It is easy to convey an idea of this magnitude in a form more realisable, by giving it a somewhat mechanical turn. Suppose that the energy in a ton of radium could be utilised in thirty years, instead of being evolved at its invariable slow rate of 1,760 years for half-disintegration, it would suffice to propel a ship of 15,000 tons, with engines of 15,000 horse-power, at the rate of 15 knots an hour, for 30 years—practically the lifetime of the ship. To do this actually requires a million and a half tons of coal.

It is easily seen that the virtue of the energy of the radium consists in the small weight in which it is contained; in other words, the radium-energy is in an enormously concentrated form. I have attempted to apply the energy contained in niton to various purposes; it decomposes water, ammonia, hydrogen chloride, and carbon dioxide, each into its constituents; further experiments on its action on salts of copper appeared to show that the metal copper was converted partially into lithium, a metal of the sodium column; and similar experiments, of which there is not time to speak, indicate that thorium, zirconium, titanium, and silicon are degraded into carbon; for solutions of compounds of these, mixed with niton, invariably generated carbon dioxide; while cerium, silver, mercury, and some other metals gave none. One can imagine the very atoms themselves, exposed to bombardment by enormously, quickly moving helium atoms failing to withstand the impacts. Indeed, the argument *a priori* is a strong one; if we know for certain that radium and its descendants decompose spontaneously, evolving energy, why should not other more stable elements decompose when subjected to enormous strains?

This leads to the speculation whether, if elements are capable of disintegration, the world may not have at its disposal a hitherto unsuspected source of energy. If radium were to evolve its stored-up energy at the same rate that gun-cotton does, we should have an undreamt-of explosive; could we control the rate we should have a useful and potent source of energy, provided always that a sufficient supply of radium were forthcoming. But the supply is certainly a very limited one; and it can be safely affirmed that the production will never surpass half an ounce a year. If, however, the elements which we have been used to consider as permanent are capable of changing with evolution of energy; if some form of catalyser could be discovered which would usefully increase their almost inconceivably slow rate of change, then it is not too much to say that the whole future of our race would be altered.

The whole progress of the human race has indeed been due to individual members discovering means of concentrating energy, and

of transforming one form into another. The carnivorous animals strike with their paws and crush with their teeth; the first man who aided his arm with a stick in striking a blow discovered how to concentrate his small supply of kinetic energy; the first man who used a spear found that its sharp point in motion represented a still more concentrated form; the arrow was a further advance, for the spear was then propelled by mechanical means; the bolt of the crossbow, the bullet shot forth by compressed hot gas, first derived from black powder, later, from high explosives; all these represent progress. To take another sequence: the preparation of oxygen by Priestley applied energy to oxide of mercury in the form of heat; Davy improved on this when he concentrated electrical energy into the tip of a thin wire by aid of a powerful battery, and isolated potassium and sodium.

Great progress has been made during the past century in effecting the conversion of one form of energy into others, with as little useless expenditure as possible. Let me illustrate by examples: A good steam engine converts about one-eighth of the potential energy of the fuel into useful work; seven-eighths are lost as unused heat, and useless friction. A good gas-engine utilises more than one-third of the total energy in the gaseous fuel; two-thirds are uneconomically expended. This is a universal proposition; in order to effect the conversion from one form of energy into another, some energy must be expended uneconomically. If A is the total energy which it is required to convert; if B is the energy into which it is desired to convert A ; then a certain amount of energy, C , must be expended to effect the conversion. In short, $A = B + C$. It is eminently desirable to keep C , the useless expenditure, as small as possible; it can never equal zero, but it can be made small. The ratio of C to B (the economic coefficient) should therefore be as large as is attainable.

The middle of the nineteenth century will always be noted as the beginning of the golden age of science; the epoch when great generalisations were made, of the highest importance on all sides, philosophical, economic, and scientific. Carnot, Clausius, Helmholtz, Julius Robert Mayer abroad, and the Thomsons, Lord Kelvin and his brother James, Rankine, Tait, Joule, Clerk Maxwell and many others at home, laid the foundations on which the splendid structure has been erected. That the latent energy of fuel can be converted into energy of motion by means of the steam engine is what we owe to Newcomen and Watt; that the kinetic energy of the fly-wheel can be transformed into electrical energy was due to Faraday, and to him, too, we are indebted for the re-conversion of electrical energy into mechanical work; and it is this power of work which gives us leisure, and which enables a small country like ours to support the population which inhabits it.

I suppose that it will be generally granted that the Commonwealth

of Athens attained a high-water mark in literature and thought, which has never yet been surpassed. The reason is not difficult to find; a large proportion of its people had ample leisure, due to ample means; they had time to think, and time to discuss what they thought. How was this achieved? The answer is simple: each Greek Freeman had on an average at least five helots who did his bidding, who worked his mines, looked after his farm, and, in short, saved him from manual labour. Now, we in Britain are much better off; the population of the British Isles is in round numbers 45 millions; there are consumed in our factories at least 50 million tons of coal annually, and 'it is generally agreed that the consumption of coal per indicated horse-power per hour is on an average about 5 lb.' (Royal Commission on Coal Supplies, Part I.). This gives seven million horse-power per year. How many man-power are equal to a horse-power? I have arrived at an estimate thus: A Bhutanese can carry 230 lb. *plus* his own weight, in all 400 lb., up a hill 4,000 feet high in eight hours; this is equivalent to about one-twenty-fifth of a horse-power; seven million horse-power are therefore about 175 million man-power. Taking a family as consisting on the average of five persons, our 45 millions would represent nine million families; and dividing the total man-power by the number of families, we must conclude that each British family has, on the average, nearly twenty 'helots' doing his bidding, instead of the five of the Athenian family. We do not appear, however, to have gained more leisure thereby, but it is this that makes it possible for the British Isles to support the population which it does.

We have in this world of ours only a limited supply of stored-up energy; in the British Isles a very limited one—namely, our coalfields. The rate at which this supply is being exhausted has been increasing very steadily for the last forty years, as anyone can prove by mapping the data given on page 27, table D, of the General Report of the Royal Commission on Coal Supplies (1906). In 1870 110 million tons were mined in Great Britain, and ever since the amount has increased by three and a-third million tons a year. The available quantity of coal in the proved coalfields is very nearly 100,000 million tons; it is easy to calculate that if the rate of working increases as it is doing our coal will be completely exhausted in 175 years. But, it will be replied, the rate of increase will slow down. Why? It has shown no sign whatever of slackening during the last forty years. Later, of course, it must slow down, when coal grows dearer owing to approaching exhaustion. It may also be said that 175 years is a long time; why, I myself have seen a man whose father fought in the '45 on the Pretender's side, nearly 170 years ago! In the life of a nation 175 years is a span.

This consumption is still proceeding at an accelerated rate. Between 1905 and 1907 the amount of coal raised in the United Kingdom in-

creased from 236 to 268 million tons, equal to six tons per head of the population, against three and a half tons in Belgium, two and a half tons in Germany, and one ton in France. Our commercial supremacy and our power of competing with other European nations are obviously governed, so far as we can see, by the relative price of coal; and when our prices rise, owing to the approaching exhaustion of our supplies, we may look forward to the near approach of famine and misery.

Having been struck some years ago with the optimism of my non-scientific friends as regards our future, I suggested that a committee of the British Science Guild should be formed to investigate our available sources of energy. This Guild is an organisation, founded by Sir Norman Lockyer, after his tenure of the Presidency of this Association, for the purpose of endeavouring to impress on our people and their Government the necessity of viewing problems affecting the race and the State from the standpoint of science; and the definition of science in this, as in other connections, is simply the acquisition of knowledge, and orderly reasoning on experience already gained and on experiments capable of being carried out, so as to forecast and control the course of events; and, if possible, to apply this knowledge to the benefit of the human race.

The Science Guild has enlisted the services of a number of men, each eminent in his own department, and each has now reported on the particular source of energy of which he has special knowledge.

Besides considering the uses of coal and its products, and how they may be more economically employed, in which branches the Hon. Sir Charles Parsons, Mr. Dugald Clerk, Sir Boverton Redwood, Dr. Beilby, Dr. Hele-Shaw, Prof. Vivian Lewes and others have furnished reports, the following sources of energy have been brought under review: The possibility of utilising the tides; the internal heat of the earth; the winds; solar heat; water-power; the extension of forests, and the use of wood and peat as fuels; and lastly, the possibility of controlling the undoubted but almost infinitely slow disintegration of the elements, with the view of utilising their stored-up energy.

However interesting a detailed discussion of these possible sources of energy might be, time prevents my dwelling on them. Suffice it to say that the Hon. R. J. Strutt has shown that in this country at least it would be impracticable to attempt to utilise terrestrial heat from boreholes; others have deduced that from the tides, the winds, and water-power small supplies of energy are no doubt obtainable, but that, in comparison with that derived from the combustion of coal, they are negligible; nothing is to be hoped for from the direct utilisation of solar heat in this temperate and uncertain climate; and it would be folly to consider seriously a possible supply of energy in a conceivable acceleration of the liberation of energy by atomic change. It looks utterly improbable, too, that we shall ever be able to utilise the energy due to the

revolution of the earth on her axis, or to her proper motion round the sun.

Attention should undoubtedly be paid to forestry, and to the utilisation of our stores of peat. On the Continent, the forests are largely the property of the State; it is unreasonable, especially in these latter days of uncertain tenure of property, to expect any private owner of land to invest money in schemes which would at best only benefit his descendants, but which, under our present trend of legislation, do not promise even that remote return. Our neighbours and rivals, Germany and France, spend annually 2,200,000*l.* on the conservation and utilisation of their forests; the net return is 6,000,000*l.* There is no doubt that we could imitate them with advantage. Moreover, an increase in our forests would bring with it an increase in our water-power; for without forest land rain rapidly reaches the sea, instead of distributing itself, so as to keep the supply of water regular, and so more easily utilised.

Various schemes have been proposed for utilising our deposits of peat: I believe that in Germany the peat industry is moderately profitable; but our humid climate does not lend itself to natural evaporation of most of the large amount of water contained in peat, without which processes of distillation prove barely remunerative.

We must therefore rely chiefly on our coal reserve for our supply of energy, and for the means of supporting our population; and it is to the more economical use of coal that we must look, in order that our life as a nation may be prolonged. We can economise in many ways: By the substitution of turbine engines for reciprocating engines, thereby reducing the coal required per horse-power from 4 to 5 lb. to $1\frac{1}{2}$ or 2 lb.; by the further replacement of turbines by gas engines, raising the economy to 30 per cent. of the total energy available in the coal, that is, lowering the coal consumption per horse-power to 1 or $1\frac{1}{4}$ lb.; by creating the power at the pit-mouth, and distributing it electrically, as is already done in the Tyne district. Economy can also be effected in replacing 'bee-hive' coke ovens by recovery ovens; this is rapidly being done; and Dr. Beilby calculates that in 1909 nearly six million tons of coal, out of a total of sixteen to eighteen millions, were coked in recovery ovens, thus effecting a saving of two to three million tons of fuel annually. Progress is also being made in substituting gas for coal or coke in metallurgical, chemical, and other works. But it must be remembered that for economic use, gaseous fuel must not be charged with the heavy costs of piping and distribution.

The domestic fire problem is also one which claims our instant attention. It is best grappled with from the point of view of smoke. Although the actual loss of thermal energy in the form of smoke is small—at most less than a half per cent. of the fuel consumed—

still the presence of smoke is a sign of waste of fuel and careless stoking. In works, mechanical stokers which ensure regularity of firing and complete combustion of fuel are more and more widely replacing hand-firing. But we are still utterly wasteful in our consumption of fuel in domestic fires. There is probably no single remedy applicable; but the introduction of central heating, of gas fires, and of grates which permit of better utilisation of fuel will all play a part in economising our coal. It is open to argument whether it might not be wise to hasten the time when smoke is no more by imposing a sixpenny fine for each offence; an instantaneous photograph could easily prove the offence to have been committed; and the imposition of the fine might be delayed until three warnings had been given by the police.

Now I think that what I wish to convey will be best expressed by an allegory. A man of mature years who has surmounted the troubles of childhood and adolescence without much disturbance to his physical and mental state, gradually becomes aware that he is suffering from loss of blood; his system is being drained of this essential to life and strength. What does he do? If he is sensible, he calls in a doctor, or perhaps several, in consultation; they ascertain the seat of the disease, and diagnose the cause. They point out that while consumption of blood is necessary for healthy life, it will lead to a premature end if the constantly increasing drain is not stopped. They suggest certain precautionary measures; and if he adopts them, he has a good chance of living at least as long as his contemporaries; if he neglects them, his days are numbered.

That is our condition as a nation. We have had our consultation in 1903; the doctors were the members of the Coal Commission. They showed the gravity of our case, but we have turned a deaf ear.

It is true that the self-interest of coal consumers is slowly leading them to adopt more economical means of turning coal into energy. But I have noticed and frequently publicly announced a fact which cannot but strike even the most unobservant. It is this: When trade is good, as it appears to be at present, manufacturers are making money; they are overwhelmed with orders, and have no inclination to adopt economies which do not appear to them to be essential, and the introduction of which would take thought and time, and which would withdraw the attention of their employes from the chief object of the business—how to make the most of the present opportunities. Hence improvements are postponed. When bad times come, then there is no money to spend on improvements; they are again postponed until better times arrive.

What can be done?

I would answer: Do as other nations have done and are doing;

take stock annually. The Americans have a permanent Commission initiated by Mr. Roosevelt, consisting of three representatives from each State, the sole object of which is to keep abreast with the diminution of the stores of natural energy, and to take steps to lessen its rate. This is a non-political undertaking, and one worthy of being initiated by the ruler of a great country. If the example is followed here the question will become a national one.

Two courses are open to us; first, the *laissez-faire* plan of leaving to self-interested competition the combating of waste; or second, initiating legislation which, in the interest of the whole nation, will endeavour to lessen the squandering of our national resources. This legislation may be of two kinds: penal, that is, imposing a penalty on wasteful expenditure of energy-supplies; and helpful, that is, imparting information as to what can be done, advancing loans at an easy rate of interest to enable reforms to be carried out, and insisting on the greater prosperity which would result from the use of more efficient appliances.

This is not the place, nor is there the time, to enter into detail; the subject is a complicated one, and it will demand the combined efforts of experts and legislators for a generation; but if it be not considered with the definite intention of immediate action, we shall be held up to the deserved execration of our not very remote descendants.

The two great principles which I have alluded to in an earlier part of this address must not, however, be lost sight of; they should guide all our efforts to use energy economically. Concentration of energy in the form of electric current at high potential makes it possible to convey it for long distances through thin and therefore comparatively inexpensive wires; and the economic coefficient of the conversion of mechanical into electrical, and of electrical into mechanical energy is a high one; the useless expenditure does not much exceed one-twentieth part of the energy which can be utilised. These considerations would point to the conversion at the pit-mouth of the energy of the fuel into electrical energy, using as an intermediary, turbines, or preferably gas engines; and distributing the electrical energy to where it is wanted. The use of gas engines may, if desired, be accompanied by the production of half-distilled coal, a fuel which burns nearly without smoke, and one which is suitable for domestic fires, if it is found too difficult to displace them and to induce our population to adopt the more efficient and economical systems of domestic heating which are used in America and on the Continent. The increasing use of gas for factory, metallurgical, and chemical purposes points to the gradual concentration of works near the coal mines, in order that the laying-down of expensive piping may be avoided.

An invention which would enable us to convert the energy of coal

directly into electrical energy would revolutionise our ideas and methods, yet it is not unthinkable. The nearest practical approach to this is the Mond gas-battery, which, however, has not succeeded, owing to the imperfection of the machine.

In conclusion, I would put in a plea for the study of pure science, without regard to its applications. The discovery of radium and similar radioactive substances has widened the bounds of thought. While themselves, in all probability, incapable of industrial application, save in the domain of medicine, their study has shown us to what enormous advances in the concentration of energy it is permissible to look forward, with the hope of applying the knowledge thereby gained to the betterment of the whole human race. As charity begins at home, however, and as I am speaking to the *British Association for the Advancement of Science*, I would urge that our first duty is to strive for all which makes for the permanence of the British Commonwealth, and which will enable us to transmit to our posterity a heritage not unworthy to be added to that which we have received from those who have gone before.

British Association for the Advancement of Science.

DUNDEE, 1912.

ADDRESS

PROFESSOR E. A. SCHÄFER, LL.D., D.Sc., M.D., F.R.S.,
PRESIDENT.

It is exactly forty-five years ago—to the day and hour—that the British Association last met in this city and in this hall to listen to a Presidential Address. The President was the Duke of Buccleugh; the General Secretaries, Francis Galton and T. Archer Hirst; the General Treasurer, William Spottiswoode; and the Assistant General Secretary, George Griffith, who was for many years a mainstay of the Association. The Evening Discourses were delivered by John Tyndall ‘On Matter and Force,’ by Archibald Geikie ‘On the Geological Origin of the Scenery of Scotland,’ and by Alexander Herschel ‘On the Present State of Knowledge regarding Meteors and Meteorites.’ The Presidents of Sections, which were then only seven in number, were for Mathematics and Physics, Sir William Thomson—later to be known as Lord Kelvin; for Chemistry, Thomas Anderson; for Geology, Archibald Geikie, who now as President of the Royal Society worthily fills the foremost place in science within the realm; for Biology, William Sharpey, my own revered master, to whose teaching and influence British physiology largely owes the honourable position which it at present occupies; for Geography, Sir Samuel Baker, the African explorer, who with his intrepid wife was the first to follow the Nile to its exit from the Albert Nyanza; for Economic Science, Mr. Grant Duff; and for Mechanical Science, Professor Rankine.

Other eminent men present were Sir David Brewster, J. Clerk Maxwell, Charles Wheatstone, Balfour Stewart, William Crookes, J. B. Lawes and J. H. Gilbert (names inseparable in the history of agricultural science), Crum Brown, G. D. Liveing, W. H. Russell, Alexander Williamson, Henry Alleyne Nicholson, William Allmann, John Hutton Balfour, Spencer Cobbold, Anton Dohrn, Sir John Lubbock (now Lord Avebury), William McIntosh, E. Ray Lankester,

C. W. Peach, William Pengelly, Hughes Bennett, John Cleland, John Davy, Alexander Christison, Alfred Russel Wallace, Allen Thomson, William Turner, George Busk, Michael Foster (not yet founder of the Cambridge School of Physiology), Henry Howorth, Sir Roderick Murchison, Clements R. Markham, Sir William (afterwards Lord) Armstrong, and Douglas Galton. Many of those enumerated have in the course of nature passed away from us, but not a few remain, and we are glad to know that most of these retain their ancient vigour in spite of the five-and-forty years which separate us from the last meeting in this place.

For the Address with which it is usual for the President to open the proceedings of the annual assembly, the field covered by the aims of the British Association provides the widest possible range of material from which to select. One condition alone is prescribed by custom, viz., that the subject chosen shall lie within the bounds of those branches of knowledge which are dealt with in the Sections. There can be no ground of complaint regarding this limitation on the score of variety, for within the forty years that I have myself been present (not, I regret to say, without a break) at these gatherings, problems relating to the highest mathematics on the one hand, and to the most utilitarian applications of science on the other, with every possible gradation between these extremes, have been discussed before us by successive Presidents; and the addition from time to time of new Sections (one of which, that of Agriculture, we welcome at this Meeting) enables the whilom occupant of this chair to traverse paths which have not been previously trodden by his predecessors. On the last two occasions, under the genial guidance of Professors Bonney and Sir William Ramsay, we have successively been taken in imagination to the glaciers which flow between the highest peaks of the Alps and into the bowels of the earth; where we were invited to contemplate the prospective disappearance of the material upon which all our industrial prosperity depends. Needless to say that the lessons to be drawn from our visits to those unaccustomed levels were placed before us with all the eloquence with which these eminent representatives of Geology and Chemistry are gifted. It is fortunately not expected that I should be able to soar to such heights or to plunge to such depths, for the branch of science with which I am personally associated is merely concerned with the investigation of the problems of living beings, and I am able to invite you to remain for an hour or so at the level of ordinary mortality to consider certain questions which at any rate cannot fail to have an immediate interest for every one present, seeing that they deal with the nature, origin, and maintenance of life.

Everybody knows, or thinks he knows, what life is; at least, we are

all acquainted with its ordinary, obvious manifestations. It would,

Definition. therefore, seem that it should not be difficult to find an exact definition. The quest has nevertheless baffled the most acute thinkers. Herbert Spencer devoted two chapters of his 'Principles of Biology' to the discussion of the attempts at definition which had up to that date been proposed, and himself suggested another. But at the end of it all he is constrained to admit that no expression had been found which would embrace all the known manifestations of animate, and at the same time exclude those of admittedly inanimate, objects.

The ordinary dictionary definition of life is 'the state of living.' Dastre, following Claude Bernard, defines it as 'the sum total of the phenomena common to all living beings.'¹ Both of these definitions are, however, of the same character as Sydney Smith's definition of an archdeacon as 'a person who performs archidiaconal functions.' I am not myself proposing to take up your time by attempting to grapple with a task which has proved too great for the intellectual giants of philosophy, and I have the less disposition to do so because recent advances in knowledge have suggested the probability that the dividing line between animate and inanimate matter is less sharp than it has hitherto been regarded, so that the difficulty of finding an inclusive definition is correspondingly increased.

As a mere word 'life' is interesting in the fact that it is one of those abstract terms which has no direct antithesis; although probably most persons would regard 'death' in that light. A little consideration will show that this is not the case. 'Death' implies the pre-existence of 'life'; there are physiological grounds for regarding death as a phenomenon of life—it is the completion, the last act of life. We cannot speak of a non-living object as *possessing* death in the sense that we speak of a living object as *possessing* life. The adjective 'dead' is, it is true, applied in a popular sense antithetically to objects which have never possessed life; as in the proverbial expression 'as dead as a door-nail.' But in the strict sense such application is not justifiable, since the use of the terms dead and living implies either in the past or in the present the possession of the recognised properties of living matter. On the other hand, the expressions *living* and *lifeless*, *animate* and *inanimate*, furnish terms which are undoubtedly antithetical. Strictly and literally, the words animate and inanimate express the presence or absence of 'soul'; and not infrequently we find the terms 'life' and 'soul' erroneously employed as if identical. But it is hardly necessary for me to state that the remarks I have to make regarding 'life' must not be taken to apply to the conception to which the word 'soul' is attached.

Life not identical with soul.

¹ *La vie et la mort*, English translation by W. J. Greenstreet, 1911, p. 54.

The fact that the formation of such a conception is only possible in connection with life, and that the growth and elaboration of the conception has only been possible as the result of the most complex processes of life in the most complex of living organisms, has doubtless led to a belief in the identity of life with soul. But unless the use of the expression 'soul' is extended to a degree which would deprive it of all special significance, the distinction between these terms must be strictly maintained. For the problems of life are essentially problems of matter; we cannot conceive of life in the scientific sense as existing apart from matter. The phenomena of life are investigated, and can only be investigated, by the same methods as all other phenomena of matter, and the general results of such investigations tend to show that living beings are governed by laws identical with those which govern inanimate matter. The more we study the manifestations of life the more we become convinced of the truth of this statement and the less we are disposed to call in the aid of a special and unknown form of energy to explain those manifestations.

Problems of life are problems of matter.

The most obvious manifestation of life is 'spontaneous' movement. We see a man, a dog, a bird move, and we know that they are alive.

Phenomena indicative of life: Movement.

We place a drop of pond water under the microscope, and see numberless particles rapidly moving within it; we affirm that it swarms with 'life.' We notice a small mass of clear slime changing its shape, throwing out projections of its structureless substance, creeping from one part of the field of the microscope to another. We recognise that the slime is living; we give it a name—*Amœba limax*—the slug amœba. We observe similar movements in individual cells of our own body; in the white corpuscles of our blood, in connective tissue cells, in growing nerve cells, in young cells everywhere. We denote the similarity between these movements and those of the amœba by employing the descriptive term 'amœboid' for both. We regard such movements as indicative of the possession of 'life'; nothing seems more justifiable than such an inference.

But physicists² show us movements of a precisely similar character in substances which no one by any stretch of imagination can regard as living; movements of oil drops, of organic and inorganic mixtures, even of mercury globules, which are indistinguishable in their character from those of the living organisms we have been studying: movements which can only be described by the same term amœboid, yet obviously produced as the result of purely physical and chemical reactions causing changes in surface tension of the fluids under exami-

Similarity of movements in living and non-living matter.

² G. Quincke, *Annal. d. Physik u. Chem.* 1870 and 1888.

nation.³ It is therefore certain that such movements are not specifically 'vital,' that their presence does not necessarily denote 'life.' And when we investigate closely even such active movements as those of a vibratile cilium or a phenomenon so closely identified with life as the contraction of a muscle, we find that these present so many analogies with amoeboid movements as to render it certain that they are fundamentally of the same character and produced in much the same manner.⁴ Nor can we for a moment doubt that the complex actions which are characteristic of the more highly differentiated organisms have been developed in the course of evolution from the simple movements characterising the activity of undifferentiated protoplasm; movements which can themselves, as we have seen, be perfectly imitated by non-living material. The chain of evidence regarding this particular manifestation of life—movement—is complete. Whether exhibited as the amoeboid movement of the proteus animalcule or of the white corpuscle of our blood; as the ciliary motion of the infusorian or of the ciliated cell; as the contraction of a muscle under the governance of the will, or as the throbbing of the human heart responsive to every emotion of the mind, we cannot but conclude that it is alike subject to and produced in conformity with the general laws of matter, by agencies resembling those which cause movements in lifeless material.⁵

It will perhaps be contended that the resemblances between the movements of living and non-living matter may be only superficial, and that the conclusion regarding their identity to which we are led will be dissipated when we endeavour to penetrate more deeply into the working of living substance. For can we not recognise along with the possession of movement the presence of other phenomena which are

**Assimilation
and disas-
similation.**

equally characteristic of life and with which non-living material is not endowed? Prominent among the characteristic phenomena of life are the processes of assimilation and disassimilation, the taking in of food and its elabora-

³ The causation not only of movements but of various other manifestations of life by alterations in surface tension of living substance is ably dealt with by A. B. Macallum in a recent article in Asher and Spiro's *Ergebnisse der Physiologie*, 1911. Macallum has described an accumulation of potassium salts at the more active surfaces of the protoplasm of many cells, and correlates this with the production of cell-activity by the effect of such accumulation upon the surface tension. The literature of the subject will be found in this article.

⁴ G. F. Fitzgerald (*Brit. Assoc. Reports*, 1898, and *Scient. Trans. Roy. Dublin Society*, 1898) arrived at this conclusion with regard to muscle from purely physical considerations.

⁵ 'Vital spontaneity, so readily accepted by persons ignorant of biology, is disproved by the whole history of science. Every vital manifestation is a response to a stimulus, a provoked phenomenon. It is unnecessary to say this is also the case with brute bodies, since that is precisely the foundation of the great principle of the inertia of matter. It is plain that it is also as applicable to living as to inanimate matter.'—Dastre, *op. cit.* p. 230.

tion.⁶ These, surely, it may be thought, are not shared by matter which is not endowed with life. Unfortunately for this argument, similar processes occur characteristically in situations which no one would think of associating with the presence of life. A striking example of this is afforded by the osmotic phenomena presented by solutions separated from one another by semipermeable membranes, or films, a condition which is precisely that which is constantly found in living matter.⁷

It is not so long ago that the chemistry of organic matter was thought to be entirely different from that of inorganic substances. But the line between inorganic and organic chemistry, which up to the middle of the last century appeared sharp, subsequently became misty and has now disappeared. Similarly the chemistry of living organisms, which is now a recognised branch of organic chemistry, but used to be considered as so much outside the domain of the chemist that it could only be dealt with by those whose special business it was to study 'vital' processes, is passing every day more out of the hands of the biologist and into those of the pure chemist.

Somewhat more than half a century ago Thomas Graham published his epoch-making observations relating to the properties of matter in the colloidal state: observations which are proving all-important in assisting our comprehension of the properties of living substance. For it is becoming every day more apparent that the chemistry and physics of the living organism are essentially the chemistry and physics of nitrogenous colloids. Living substance or protoplasm always, in fact, takes the form of a colloidal solution. In this solution the colloids are associated with crystalloids (electrolytes), which are either free in the solution or attached to the molecules of the colloids. Surrounding and enclosing the living substance thus constituted of both colloid and crystalloid material is a film, probably also formed of colloid, but which may have a lipid substratum associated with it (Overton). This film serves the purpose of an osmotic membrane, permitting of exchanges by diffusion between the colloidal solution constituting the protoplasm

Chemical phenomena accompanying life.

The colloid constitution of living matter. Identity of physical and chemical processes in living and non-living matter.

⁶ The terms 'assimilation' and 'disassimilation' express the physical and chemical changes which occur within protoplasm as the result of the intake of nutrient material from the circumambient medium and its ultimate transformation into waste products which are passed out again into that medium; the whole cycle of these changes being embraced under the term 'metabolism.'

⁷ Leduc (*The Mechanism of Life*, English translation by W. Deane Butcher, 1911) has given many illustrations of this statement. In the Report of the meeting of 1867 in Dundee is a paper by Dr. J. D. Heaton (On Simulations of Vegetable Growths by Mineral Substances) dealing with the same class of phenomena. The conditions of osmosis in cells have been especially studied by Hamburger (*Osmotischer Druck und Ionenlehre*, Wiesbaden, 1902-4).

PRESIDENT'S ADDRESS.

and the circumambient medium in which it lives. Other similar films or membranes occur in the interior of protoplasm. These films have in many cases specific characters, both physical and chemical, thus favouring the diffusion of special kinds of material into and out of the protoplasm and from one part of the protoplasm to another. It is the changes produced under these physical conditions, associated with those caused by active chemical agents formed within protoplasm and known as *enzymes*, that effect assimilation and disassimilation. Quite similar changes can be produced outside the body (*in vitro*) by the employment of methods of a purely physical and chemical nature. It is true that we are not yet familiar with all the intermediate stages of transformation of the materials which are taken in by a living body into the materials which are given out from it. But since the initial processes and the final results are the same as they would be on the assumption that the changes are brought about in conformity with the known laws of chemistry and physics, we may fairly conclude that all changes in living substance are brought about by ordinary chemical and physical forces.

Should it be contended that growth and reproduction are properties possessed only by living bodies and constitute a test by which we may differentiate between life and non-life, between the animate and inanimate creation, it must be replied that no contention can be more fallacious. Inorganic crystals grow and multiply and reproduce their like, given a supply of the requisite pabulum. In most cases for each kind of crystal there is, as with living organisms, a limit of growth which is not exceeded, and further increase of the crystalline matter results not in further increase in size but in multiplication of similar crystals. Leduc has shown that the growth and division of artificial colloids of an inorganic nature, when placed in an appropriate medium, present singular resemblances to the phenomena of the growth and division of living organisms. Even so complex a process as the division of a cell-nucleus by karyokinesis as a preliminary to the multiplication of the cell by division—a phenomenon which would *prima facie* have seemed and has been commonly regarded as a distinctive manifestation of the life of the cell—can be imitated with solutions of a simple inorganic salt, such as chloride of sodium, containing a suspension of carbon particles; which arrange and rearrange themselves under the influence of the movements of the electrolytes in a manner indistinguishable from that adopted by the particles of chromatin in a dividing nucleus. And in the process of sexual reproduction, the researches of J. Loeb and others upon the ova of the sea-urchin have proved that we can no longer consider such an apparently vital phenomenon as the fertilisation of the egg as being the result of living

Similarity of the processes of growth and reproduction in living and non-living matter.

material brought to it by the spermatozoon, since it is possible to start the process of division of the ovum and the resulting formation of cells, and ultimately of all the tissues and organs—in short, to bring about the development of the whole body—if a simple chemical reagent is substituted for the male element in the process of fertilisation. Indeed, even a mechanical or electrical stimulus may suffice to start development. *Kurz und gut*, as the Germans say, vitalism

The question of vitalism and vital force.

as a working hypothesis has not only had its foundations undermined, but most of the superstructure has toppled over, and if any difficulties of explanation still persist, we are justified in assuming that the cause is to be found in our imperfect knowledge of the constitution and working of living material. At the best vitalism explains nothing, and the term 'vital force' is an expression of ignorance which can bring us no further along the path of knowledge. Nor is the problem in any way advanced by substituting for the term 'vitalism' 'neo-vitalism,' and for 'vital force' 'biotic energy.'^s 'New presbyter is but old priest writ large.'

Further, in its chemical composition we are no longer compelled to consider living substance as possessing infinite complexity, as was thought to be the case when chemists first began to break up the proteins of the body into their simpler constituents. The researches of Miescher, which have been continued and elaborated by Kossel and his pupils, have

The possibility of the synthesis of living matter.

acquainted us with the fact that a body so important for the nutritive and reproductive functions of the cell as the nucleus—which may be said indeed to represent the quintessence of cell-life—possesses a chemical constitution of no very great complexity; so that we may even hope some day to see the material which composes it prepared synthetically. And when we consider that the nucleus is not only itself formed of living substance, but is capable of causing other living substance to be built up; is, in fact, the directing agent in all the principal chemical changes which take place within the living cell, it must be admitted that we are a long step forward in our knowledge of the chemical basis of life. That it is the *form* of nuclear matter rather than its chemical and molecular structure which is the important factor in nuclear activity cannot be supposed. The form of nuclei, as every microscopist knows, varies infinitely, and there are numerous living organisms in which the nuclear matter is without form, appearing simply as granules distributed in the protoplasm. Not that the form assumed and the transformations undergone by the nucleus are without import-

^s B. Moore, in *Recent Advances in Physiology*, 1906; Moore and Roaf, *ibid.*; and *Further Advances in Physiology*, 1909. Moore lays especial stress on the transformations of energy which occur in protoplasm. See on the question of vitalism Gley (*Revue Scientifique*, 1911) and D'Arcy Thompson (Address to Section D at Portsmouth, 1911).

ance; but it is none the less true that even in an amorphous condition the material which in the ordinary cell takes the form of a 'nucleus' may, in simpler organisms which have not in the process of evolution become complete cells, fulfil functions in many respects similar to those fulfilled by the nucleus of the more differentiated organism.

A similar anticipation regarding the probability of eventual synthetic production may be made for the proteins of the cell-substance. Considerable progress in this direction has indeed already been made by Emil Fischer, who has for many years been engaged in the task of building up the nitrogenous combinations which enter into the formation of the complex molecule of protein. It is satisfactory to know that the significance of the work both of Fischer and of Kossel in this field of biological chemistry has been recognised by the award to each of these distinguished chemists of a Nobel prize.

The elements composing living substance are few in number. Those which are constantly present are carbon, hydrogen, oxygen, and nitrogen.

With these, both in nuclear matter and also, but to a less degree, in the more diffuse living material which we know as protoplasm, phosphorus is always associated. 'Ohne Phosphor kein Gedank' is an accepted aphorism; 'Ohne Phosphor kein Leben' is equally true. Moreover, a large proportion, rarely less than 70 per cent., of water appears essential for any manifestation of life, although not in all cases necessary for its continuance, since organisms are known which will bear the loss of the greater part if not the whole of the water they contain without permanent impairment of their vitality. The presence of certain inorganic salts is no less essential, chief amongst them being chloride of sodium and salts of calcium, magnesium, potassium, and iron. The combination of these elements into a colloidal compound represents the chemical basis of life; and when the chemist succeeds in building up this compound it will without doubt be found to exhibit the phenomena which we are in the habit of associating with the term 'life.'^{*}

The above considerations seem to point to the conclusion that the possibility of the production of life—i.e., of living material—is not so remote as has been generally assumed. Since the experiments of Pasteur, few have ventured to affirm a belief in the spontaneous generation of bacteria and monads and other micro-organisms, although before his time this was by many believed to be of universal occurrence. My esteemed friend Dr. Charlton Bastian is, so far as I am aware, the only scientific man of eminence who still adheres to the old creed, and Dr. Bastian, in spite of numerous experiments and the publication of many

The chemical constitution of living substance.

Source of life. The possibility of spontaneous generation.

^{*} The most recent account of the chemistry of protoplasm is that by Botazzi (*Das Cytoplasma u. die Körpersäfte*) in Winterstein's *Handb. d. vergl. Physiologie*, Bd. I., 1912. The literature is given in this article.

books and papers, has not hitherto succeeded in winning over any converts to his opinion. I am myself so entirely convinced of the accuracy of the results which Pasteur obtained—are they not within the daily and hourly experience of everyone who deals with the sterilisation of organic solutions?—that I do not hesitate to believe, if living *torulae* or mycelia are exhibited to me in flasks which had been subjected to prolonged boiling after being hermetically sealed, that there has been some fallacy either in the premisses or in the carrying out of the operation. The appearance of organisms in such flasks would not furnish to my mind proof that they were the result of spontaneous generation. Assuming no fault in manipulation or fallacy in observation, I should find it simpler to believe that the germs of such organisms have resisted the effects of prolonged heat than that they became generated spontaneously. If spontaneous generation is possible, we cannot expect it to take the form of living beings which show so marked a degree of differentiation, both structural and functional, as the organisms which are described as making their appearance in these experimental flasks.¹⁰ Nor should we expect the spontaneous generation of living substance of any kind to occur in a fluid the organic constituents of which have been so altered by heat that they can retain no sort of chemical resemblance to the organic constituents of living matter. If the formation of life—of living substance—is possible at the present day—and for my own part I see no reason to doubt it—a boiled infusion of organic matter—and still less of inorganic matter—is the last place in which to look for it. Our mistrust of such evidence as has yet been brought forward need not, however, preclude us from admitting the possibility of the formation of living from non-living substance.¹¹

Setting aside, as devoid of scientific foundation, the idea of immediate

¹⁰ It is fair to point out that Dr. Bastian suggests that the formation of ultramicroscopic living particles may precede the appearance of the microscopic organisms which he describes. *The Origin of Life*, 1911, p. 65.

¹¹ The present position of the subject is succinctly stated by Dr. Chalmers Mitchell in his article on 'Abiogenesis' in the *Encyclopædia Britannica*. Dr. Mitchell adds: 'It may be that in the progress of science it may yet be possible to construct living protoplasm from non-living material. The refutation of abiogenesis has no further bearing on this possibility than to make it probable that if protoplasm ultimately be formed in the laboratory, it will be by a series of steps, the earlier steps being the formation of some substance, or substances, now unknown, which are not protoplasm. Such intermediate stages may have existed in the past.' And Huxley in his Presidential Address at Liverpool in 1870 says: 'But though I cannot express this conviction' (i.e., of the impossibility of the occurrence of abiogenesis, as exemplified by the appearance of organisms in hermetically sealed and sterilised flasks) 'too strongly I must carefully guard myself against the supposition that I intend to suggest that no such thing as abiogenesis has ever taken place in the past or ever will take place in the future. With organic chemistry, molecular physics and physiology yet in their infancy and every day making prodigious strides, I think it would be the height of presumption for any man to say that the conditions under which matter assumes the properties we call "vital" may not, some day, be artificially brought together.'

supernatural intervention in the first production of life, we are not only justified in believing, but compelled to believe, that living matter must have owed its origin to causes similar in character to those which have been instrumental in producing all other forms of matter in the universe; in other words, to a process of gradual evolution.¹² But it has been customary of late amongst biologists to shelve the investigation of the mode of origin of life by evolution from non-living matter by relegating its solution to some former condition of the earth's history, when, it is assumed, opportunities were accidentally favourable for the passage of inanimate matter into animate; such opportunities, it is also assumed, having never since recurred and being never likely to recur.¹³

Various eminent scientific men have even supposed that life has not actually originated upon our globe, but has been brought to it from another planet or from another stellar system. Some of my audience may still remember the controversy that was excited when the theory of the origin of terrestrial life by the intermediation of a meteorite was propounded by Sir William Thomson in his Presidential Address at the meeting of this Association in Edinburgh in 1871. To this 'meteorite' theory¹⁴ the apparently fatal objection was raised that it would take some sixty million years for a meteorite to travel from the nearest stellar system to our earth, and it is inconceivable that any kind of life could be maintained during such a period. Even from the nearest planet 150 years would be necessary, and the heating of the meteorite in passing through our atmosphere and at its impact with the earth would, in all probability, destroy any life which might have existed within it. A cognate theory, that of *cosmic panspermia*, assumes that life may exist and may have existed indefinitely in cosmic dust in the interstellar spaces (Richter, 1865; Cohn, 1872), and may with this dust fall slowly to the earth without undergoing the heating which is experienced by a meteorite. Arrhenius,¹⁵ who adopts this theory, states that if living germs were carried through the ether by luminous and other radiations the time necessary for their transportation from our globe to the nearest stellar system would be only nine thousand years, and to Mars only twenty days!

¹² The arguments in favour of this proposition have been arrayed by Meldola in his Herbert Spencer Lecture, 1910, pp. 16-24. Meldola leaves the question open whether such evolution has occurred only in past years or is also taking place now. He concludes that whereas certain carbon compounds have survived by reason of possessing extreme stability, others—the precursors of living matter—survived owing to the possession of extreme lability and adaptability to variable conditions of environment. A similar suggestion was previously made by Lockyer, *Inorganic Evolution*, 1900, pp. 169, 170.

¹³ T. H. Huxley, Presidential Address, 1870; A. B. Macallum, 'On the Origin of Life on the Globe,' in *Trans. Canadian Institute*, VIII.

¹⁴ First suggested, according to Dastre, by de Salles-Guyon (Dastre, *op. cit.*, p. 252). The theory received the support of Helmholtz.

¹⁵ *Worlds in the Making*, transl. by H. Borns, chap. viii., p. 221, 1908.

But the acceptance of such theories of the arrival of life on the earth does not bring us any nearer to a conception of its actual mode of origin; on the contrary it merely serves to banish the investigation of the question to some conveniently inaccessible corner of the universe and leaves us in the unsatisfactory position of affirming not only that we have no knowledge as to the mode of origin of life—which is unfortunately true—but that we never can acquire such knowledge—which it is to be hoped is not true.¹⁶ Knowing what we know, and believing what we believe, as to the part played by evolution in the development of terrestrial matter, we are, I think (without denying the possibility of the existence of life in other parts of the universe¹⁷) justified in regarding these cosmic theories as inherently improbable—at least in comparison with the solution of the problem which the evolutionary hypothesis offers.¹⁸

I assume that the majority of my audience have at least a general idea of the scope of this hypothesis, the general acceptance of which has within the last sixty years altered the whole aspect not only of biology, but of every other branch of natural science, including astronomy, geology, physics, and chemistry.¹⁹ To those who have not this familiarity I would recommend the perusal of a little book by Professor Judd entitled 'The Coming of Evolution,' which has recently appeared as one of the Cambridge manuals. I know of no similar book in which the subject is as clearly and succinctly treated. Although the author nowhere expresses the opinion that the actual origin of life on the earth has arisen by evolution from non-living matter, it is impossible to read either this or any similar exposition in which the essential unity of the evolutionary process is insisted upon

The evolutionary hypothesis as applied to the origin of life.

¹⁶ 'The history of science shows how dangerous it is to brush aside mysteries—i.e., unsolved problems—and to interpose the barrier placarded "eternal—no thoroughfare."'—R. Meldola, Herbert Spencer Lecture, 1910.

¹⁷ Some authorities, such as Errera, contend, with much probability, that the conditions in interstellar space are such that life, as we understand it, could not possibly exist there.

¹⁸ As Verworn points out, such theories would equally apply to the origin of any other chemical combination, whether inorganic or organic, which is met with on our globe, so that they lead directly to absurd conclusions.—*Allgemeine Physiologie*, 1911.

¹⁹ As Meldola insists, this general acceptance was in the first instance largely due to the writings of Herbert Spencer: 'We are now prepared for evolution in every domain. . . . As in the case of most great generalisations, thought had been moving in this direction for many years: . . . Lamarck and Buffon had suggested a definite mechanism of organic development, Kant and Laplace a principle of celestial evolution, while Lyell had placed geology upon an evolutionary basis. The principle of continuity was beginning to be recognised in physical science. . . . It was Spencer who brought these independent lines of thought to a focus, and who was the first to make any systematic attempt to show that the law of development expressed in its widest and most abstract form was universally followed throughout cosmical processes, inorganic, organic, and super-organic.'—*Op. cit.*, p. 14.

without concluding that the origin of life must have been due to the same process, this process being; without exception, continuous, and admitting of no gap at any part of its course. Looking therefore at the evolution of living matter by the light which is shed upon it from the study of the evolution of matter in general, we are led to regard it as having been produced, not by a sudden alteration, whether exerted by natural or supernatural agency, but by a gradual process of change from material which was lifeless, through material on the borderland between inanimate and animate, to material which has all the characteristics to which we attach the term 'life.' So far from expecting a sudden leap from an inorganic, or at least an unorganised, into an organic and organised condition, from an entirely inanimate substance to a completely animate state of being, should we not rather expect a gradual procession of changes from inorganic to organic matter, through stages of gradually increasing complexity until material which can be termed living is attained? And in place of looking for the production of fully formed living organisms in hermetically sealed flasks, should we not rather search Nature herself, under natural conditions, for evidence of the existence, either in the past or in the present, of transitional forms between living and non-living matter?

The difficulty, nay the impossibility, of obtaining evidence of such evolution from the past history of the globe is obvious. Both the hypothetical transitional material and the living material which was originally evolved from it may, as Macallum has suggested, have taken the form of diffused ultra-microscopic particles of living substance²⁰; and even if they were not diffused but aggregated into masses, these masses could have been physically nothing more than colloidal watery slime which would leave no impress upon any geological formation. Myriads of years may have elapsed before some sort of skeleton in the shape of calcareous or siliceous spicules began to evolve itself, and thus enabled 'life,' which must already have possessed a prolonged existence, to make any sort of geological record. It follows that in attempting to pursue the evolution of living matter to its beginning in terrestrial history we can only expect to be confronted with a blank wall of nescience.

The problem would appear to be hopeless of ultimate solution, if we are rigidly confined to the supposition that the evolution of life has only occurred once in the past history of the globe. But are we justified in assuming that at one period only, and as it were by a fortunate and fortuitous concomitance of substance and circumstance, living matter became evolved out of non-living matter—life became

²⁰ There still exist in fact forms of life which the microscope cannot show us (E. A. Minckin, Presidential Address to Quekett Club, 1911), and germs which are capable of passing through the pores of a Chamberland filter.

established? Is there any valid reason to conclude that at some previous period of its history our earth was more favourably circumstanced for the production of life than it is now?²¹ I have vainly sought for such reason, and if none be forthcoming the conclusion forces itself upon us that the evolution of non-living into living substance has happened more than once—and we can be by no means sure that it may not be happening still.

It is true that up to the present there is no evidence of such happening: no process of transition has hitherto been observed. But on the other hand, is it not equally true that the kind of evidence which would be of any real value in determining this question has not hitherto been looked for? We may be certain that if life is being produced from non-living substance it will be life of a far simpler character than any that has yet been observed—in material which we shall be uncertain whether to call animate or inanimate, even if we are able to detect it at all, and which we may not be able to visualise physically even after we have become convinced of its existence.²² But we can look with the mind's eye and follow in imagination the transformation which non-living matter may have undergone and may still be undergoing to produce living substance. No principle of evolution is better founded than that insisted upon by Sir Charles Lyell, justly termed by Huxley 'the greatest geologist of his time,' that we must interpret the past history of our globe by the present; that we must seek for an explanation of what has happened by the study of what is happening; that, given similar circumstances, what has occurred at one time will probably occur at another. The process of evolution is universal. The inorganic materials of the globe are continually undergoing transition. New chemical combinations are constantly being formed and old ones broken up; new elements are making their appearance and old elements disappearing.²³ Well may we ask ourselves why the production of living matter alone should be subject to other laws than those which have produced, and are producing, the various forms of non-living matter; why what has happened may not happen? If living matter has been evolved from lifeless in the past, we are justified in accepting the

²¹ Chalmers Mitchell (Article 'Life,' *Encycl. Brit.*, eleventh edition) writes as follows: 'It has been suggested from time to time that conditions very unlike those now existing were necessary for the first appearance of life, and must be repeated if living matter is to be reconstituted artificially. No support for such a view can be derived from observations of the existing conditions of life.'

²² 'Spontaneous generation of life could only be perceptibly demonstrated by filling in the long terms of a series between the simplest forms of inorganic and the simplest forms of organic substance. Were this done, it is quite possible that we should be unable to say (especially considering the vagueness of our definitions of life) where life began or ended.'—K. Pearson, *Grammar of Science*, second edition, 1900, p. 350.

²³ See on the production of elements, W. Crookes, Address to Section B, Brit. Assoc., 1886; T. Preston, *Nature*, vol. lx., p. 180; J. J. Thomson, *Phil. Mag.*, 1897, p. 311; Norman Lockyer, *op. cit.*, 1900; G. Darwin, Pres. Addr. Brit. Assoc., 1905.

conclusion that its evolution is possible in the present and in the future. Indeed, we are not only justified in accepting this conclusion, we are forced to accept it. When or where such change from non-living to living matter may first have occurred, when or where it may have continued, when or where it may still be occurring, are problems as difficult as they are interesting, but we have no right to assume that they are insoluble.

Since living matter always contains water as its most abundant constituent, and since the first living organisms recognisable as such in the geological series were aquatic, it has generally been assumed that life must first have made its appearance in the depths of the ocean.²⁴ Is it, however, certain that the assumption that life originated in the sea is correct? Is not the land-surface of our globe quite as likely to have been the nidus for the evolutionary transformation of non-living into living material as the waters which surround it? Within this soil almost any chemical transformation may occur; it is subjected much more than matters dissolved in sea-water to those fluctuations of moisture, temperature, electricity, and luminosity which are potent in producing chemical changes. But whether life, in the form of a simple slimy colloid, originated in the depths of the sea or on the surface of the land, it would be equally impossible for the geologist to trace its beginnings, and were it still becoming evolved in the same situations, it would be almost as impossible for the microscopist to follow its evolution. We are therefore not likely to obtain direct evidence regarding such a transformation of non-living into living matter in Nature, even if it is occurring under our eyes.

An obvious objection to the idea that the production of living matter from non-living has happened more than once is that, had this been the case, the geological record should reveal more than one palæontological series. This objection assumes that evolution would in every case take an exactly similar course and proceed to the same goal—an assumption which is, to say the least, improbable. If, as might well be the case, in any other palæontological series than the one with which we are acquainted the process of evolution of living beings did not proceed beyond Protista, there would be no obvious geological evidence regarding it; such evidence would only be discoverable by a carefully directed search made with that particular object in view.²⁵ I would not by any means minimise the difficulties which attend the suggestion that the

²⁴ For arguments in favour of the first appearance of life having been in the sea, see A. B. Macallum, 'The Palæochemistry of the Ocean,' *Trans. Canad. Instit.*, 1903-4.

²⁵ Lankester (Art. 'Protozoa,' *Encycl. Brit.*, tenth edition) conceives that the first protoplasm fed on the antecedent steps in its own evolution. F. J. Allen (*Brit. Assoc. Reports*, 1896) comes to the conclusion that living substance is probably constantly being produced, but that this fails to make itself evident

evolution of life may have occurred more than once or may still be happening, but on the other hand, it must not be ignored that those which attend the assumption that the production of life has occurred once only are equally serious. Indeed, had the idea of the possibility of a multiple evolution of living substance been first in the field, I doubt if the prevalent belief regarding a single fortuitous production of life upon the globe would have become established among biologists—so much are we liable to be influenced by the impressions we receive in scientific childhood!

Assuming the evolution of living matter to have occurred—whether once only or more frequently matters not for the moment—and in the form suggested, viz., as a mass of colloidal slime possessing the property of assimilation and therefore of growth, reproduction would follow as a matter of course. For all material of this physical nature—fluid or semi-fluid in character—has a tendency to undergo subdivision when its bulk exceeds a certain size. The subdivision may be into equal or nearly equal parts, or it may take the form of buds. In either case every separated part would resemble the parent in chemical and physical properties, and would equally possess the property of taking in and assimilating suitable material from its liquid environment, growing in bulk and reproducing its like by subdivision. *Omne vivum e vivo*. In this way from any beginning of living material a primitive form of life would spread, and would gradually people the globe. The establishment of life being once effected, all forms of organisation follow under the inevitable laws of evolution. *Ce n'est que le premier pas qui coûte*.

We can trace in imagination the segregation of a more highly phosphorised portion of the primitive living matter, which we may now consider to have become more akin to the protoplasm of organisms with which we are familiar. This more phosphorised portion might not for myriads of generations take the form of a definite nucleus, but it would be composed of material having a composition and qualities similar to those of the nucleus of a cell. Prominent among these qualities is that of catalysis—the function of effecting profound chemical changes in other material in contact with it without itself undergoing permanent change. This catalytic function may have been exercised directly by the living substance or may have been carried

owing to the substance being seized and assimilated by existing organisms. He believes that 'in accounting for the first origin of life on this earth it is not necessary that, as Pflüger assumed, the planet should have been at a former period a glowing fire-ball.' He 'prefers to believe that the circumstances which support life would also favour its origin.' And elsewhere: 'Life is not an extraordinary phenomenon, not even an importation from some other sphere, but rather the actual outcome of circumstances on this earth.'

**Further
course of
evolution of
life.**

on through the agency of the enzymes already mentioned, which are also of a colloid nature but of simpler constitution than itself, and which differ from the catalytic agents employed by the chemist in the fact that they produce their effects at a relatively low temperature. In the course of evolution special enzymes would become developed for adaptation to special conditions of life, and with the appearance of these and other modifications, a process of differentiation of primitive living matter into individuals with definite specific characters gradually became established. We can conceive of the production in this way from originally undifferentiated living substance of simple differentiated organisms comparable to the lowest forms of Protista. But how long it may have taken to arrive at this stage we have no means of ascertaining. To judge from the evidence afforded by the evolution of higher organisms it would seem that a vast period of time would be necessary for even this amount of organisation to establish itself.

The next important phase in the process of evolution would be the segregation and moulding of the diffused or irregularly aggregated nuclear matter into a definite nucleus around which all the chemical activity of the organism will in future be centred. Whether this change were due to a slow and gradual process of segregation or of the nature of a jump, such as Nature does occasionally make, the result would be the advancement of the living organism to the condition of a complete nucleated cell: a material advance not only in organisation but—still more important—in potentiality for future development. Life is now embodied in the cell, and every living being evolved from this will itself be either a cell or a cell-aggregate. *Omnis cellula e cellula*.

After the appearance of a nucleus—but how long after it is impossible to conjecture—another phenomenon appeared upon the scene in the occasional exchange of nuclear substance between cells. In this manner became established the process of sexual reproduction. Such exchange in the unicellular Protista might and may occur between any two cells forming the species, but in the multicellular Metazoa it became—like other functions—specialised in particular cells. The result of the exchange is rejuvenescence; associated with an increased tendency to subdivide and to produce new individuals. This is due to the introduction of a stimulating or catalytic chemical agent into the cell which is to be rejuvenated, as is proved by the experiments of Loeb already alluded to. It is true that the chemical material introduced into the germ-cell in the ordinary process of its fertilisation by the sperm-cell is usually accompanied by the introduction of definite morphological elements which blend with others already contained within the germ-cell, and it is believed that the transmission of such morphological ele-

**Formation of
the nucleated
cell.**

**Establishment
of sexual
differences.**

ments of the parental nuclei is related to the transmission of parental qualities. But we must not be blind to the possibility that these transmitted qualities may be connected with specific chemical characters of the transmitted elements; in other words, that heredity also is one of the questions the eventual solution of which we must look to the chemist to provide.

So far we have been chiefly considering life as it is found in the simplest forms of living substance, organisms for the most part entirely microscopic and neither distinctively animal nor vegetable, which were grouped together by Haeckel as a separate kingdom of animated nature—that of Protista.

Aggregate life.

But persons unfamiliar with the microscope are not in the habit of associating the term 'life' with microscopic organisms, whether these take the form of cells or of minute portions of living substance which have not yet attained to that dignity. We most of us speak and think of life as it occurs in ourselves and other animals with which we are familiar; and as we find it in the plants around us. We recognise it in these by the possession of certain properties—movement, nutrition, growth, and reproduction. We are not aware by intuition, nor can we ascertain without the employment of the microscope, that we and all the higher living beings, whether animal or vegetable, are entirely formed of aggregates of nucleated cells, each microscopic and each possessing its own life. Nor could we suspect by intuition that what we term our life is not a single indivisible property, capable of being blown out with a puff like the flame of a candle; but is the aggregate of the lives of many millions of living cells of which the body is composed. It is but a short while ago that this cell-constitution was discovered: it occurred within the lifetime, even within the memory, of some who are still with us. What a marvellous distance we have travelled since then in the path of knowledge of living organisms! The strides which were made in the advance of the mechanical sciences during the nineteenth century, which is generally considered to mark that century as an age of unexampled progress, are as nothing in comparison with those made in the domain of biology, and their interest is entirely dwarfed by that which is aroused by the facts relating to the phenomena of life which have accumulated within the same period. And not the least remarkable of these facts is the discovery of the cell-structure of plants and animals!

Let us consider how cell-aggregates came to be evolved from organisms consisting of single cells. Two methods are possible—viz. (1) the adhesion of a number of originally separate individuals; (2) the subdivision of a single individual without the products of its subdivision breaking loose from one another. No doubt this last is the manner whereby the

Evolution of the cell-aggregate.

cell-aggregate was originally formed, since it is that by which it is still produced, and we know that the life-history of the individual is an epitome of that of the species. Such aggregates were in the beginning solid; the cells in contact with one another and even in continuity: subsequently a space or cavity became formed in the interior of the mass, which was thus converted into a hollow sphere. All the cells of the aggregate were at first perfectly similar in structure and in function; there was no subdivision of labour. All would take part in effecting locomotion; all would receive stimuli from outside; all would take in and digest nutrient matter, which would then be passed into the cavity of the sphere to serve as a common store of nourishment. Such organisms are still found, and constitute the lowest types of Metazoa. Later one part of the hollow sphere became dimpled to form a cup; the cavity of the sphere became correspondingly altered in shape. With this change in structure differentiation of function between the cells covering the outside and those lining the inside of the cup made its appearance. Those on the outside subserved locomotor functions and received and transmitted from cell to cell stimuli, physical or chemical, received by the organism; while those on the inside, being freed from such functions, tended to specialise in the direction of the inception and digestion of nutrient material; which, passing from them into the cavity of the invaginated sphere, served for the nourishment of all the cells composing the organism. The further course of evolution produced many changes of form and ever-increasing complexity of the cavity thus produced by simple invagination. Some of the cell-aggregates settled down to a sedentary life, becoming plant-like in appearance and to some extent in habit. Such organisms, complex in form but simple in structure, are the Sponges. Their several parts are not, as in the higher Metazoa, closely interdependent: the destruction of any one part, however extensive, does not either immediately or ultimately involve death of the rest: all parts function separately, although doubtless mutually benefiting by their conjunction, if only by slow diffusion of nutrient fluid throughout the mass. There is already some differentiation in these organisms, but the absence of a nervous system prevents any general co-ordination, and the individual cells are largely independent of one another.

Our own life, like that of all the higher animals, is an *aggregate life*; the life of the whole is the life of the individual cells. The life of some of these cells can be put an end to, the rest may continue to live. This is, in fact, happening every moment of our lives. The cells which cover the surface of our body, which form the scarf-skin and the hairs and nails, are constantly dying and the dead cells are rubbed off or cut away, their place being taken by others supplied from living layers beneath. But the death of these cells does not

affect the vitality of the body as a whole. They serve merely as a protection, or an ornamental covering, but are otherwise not material to our existence. On the other hand, if a few cells, such as those nerve-cells under the influence of which respiration is carried on, are destroyed or injured, within a minute or two the whole living machine comes to a standstill, so that to the bystander the patient is dead; even the doctor will pronounce life to be extinct. But this pronouncement is correct only in a special sense. What has happened is that, owing to the cessation of respiration, the supply of oxygen to the tissues is cut off. And since the manifestations of life cease without this supply, the animal or patient appears to be dead. If, however, within a short period we supply the needed oxygen to the tissues requiring it, all the manifestations of life reappear.

It is only some cells which lose their vitality at the moment of so-called 'general death.' Many cells of the body retain their individual life under suitable circumstances long after the rest of the body is dead. Notable among these are muscle-cells. McWilliam showed that the muscle-cells of the blood-vessels give indications of life several days after an animal has been killed. The muscle-cells of the heart in mammals have been revived and caused to beat regularly and strongly many hours after apparent death. In man this result has been obtained by Kuliabko as many as eighteen hours after life had been pronounced extinct: in animals after days had elapsed. Waller has shown that indications of life can be elicited from various tissues many hours and even days after general death. Sherrington observed the white corpuscles of the blood to be active when kept in a suitable nutrient fluid weeks after removal from the blood-vessels. A French histologist, Jolly, has found that the white corpuscles of the frog, if kept in a cool place and under suitable conditions, show at the end of a year all the ordinary manifestations of life. Carrell and Burrows have observed activity and growth to continue for long periods in the isolated cells of a number of tissues and organs kept under observation in a suitable medium. Carrell has succeeded in substituting entire organs obtained after death from one animal for those of another of the same species, and has thereby opened up a field of surgical treatment the limit of which cannot yet be described. It is a well-established fact that any part or organ of the body can be maintained alive for hours isolated from the rest if the blood-vessels are perfused with an oxygenated solution of salts in certain proportions (Ringer). Such revival and prolongation of the life of separated organs is an ordinary procedure in laboratories of physiology. Like all the other instances enumerated, it is based on the fact that the individual cells of an organ have a life of their own which is largely independent, so that they will continue in suitable circumstances to live, although the rest of the body to which they belonged may be dead.

But some cells, and the organs which are formed of them, are more necessary to maintain the life of the aggregate than others, on account of the nature of the functions which have become specialised in them. This is the case with the nerve-cells of the respiratory centre, since they preside over the movements which are necessary to effect oxygenation of the blood. It is also true for the cells which compose the heart, since this serves to pump oxygenated blood to all other cells of the body: without such blood most cells soon cease to live. Hence we examine respiration and heart to determine if life is present: when one or both of these are at a standstill we know that life cannot be maintained. These are not the only organs necessary for the maintenance of life, but the loss of others can be borne longer, since the functions which they subserve, although useful or even essential to the organism, can be dispensed with for a time. The life of some cells is therefore more, of others less, necessary for maintaining the life of the rest. On the other hand, the cells composing certain organs have in the course of evolution ceased to be necessary, and their continued existence may even be harmful. Wiedersheim has enumerated more than a hundred of these organs in the human body. Doubtless Nature is doing her best to get rid of them for us, and our descendants will some day have ceased to possess a vermiform appendix or a pharyngeal tonsil: until that epoch arrives we must rely for their removal on the more rapid methods of surgery!

We have seen that in the simplest multicellular organisms, where one cell of the aggregate differs but little from another, the conditions for the maintenance of the life of the whole are nearly as simple as those for individual cells. But the life of a cell-aggregate such as composes the bodies of the higher animals is maintained not only by the conditions for the maintenance of the life of the individual cell being kept favourable, but also by the co-ordination of the varied activities of the cells which form the aggregate. Whereas in the lowest Metazoa all cells of the aggregate are alike in structure and function and perform and share everything in common, in higher animals (and for that matter in the higher plants also) the cells have become specialised, and each is only adapted for the performance of a particular function. Thus the cells of the gastric glands are only adapted for the secretion of gastric juice, the cells of the villi for the absorption of digested matters from the intestine, the cells of the kidney for the removal of waste products and superfluous water from the blood, those of the heart for pumping blood through the vessels. Each of these cells has its individual life and performs its individual functions. But unless there were some sort of co-operation and subordination to the needs of the body generally, there would be sometimes too little,

**The main-
tenance of
the life of
the cell-
aggregate in
the higher
animals.
Co-ordinating
mechanisms.**

PRESIDENT'S ADDRESS.

sometimes too much gastric juice secreted; sometimes too tardy, sometimes too rapid an absorption from the intestine; sometimes too little, sometimes too much blood pumped into the arteries, and so on. As the result of such lack of co-operation the life of the whole would cease to be normal and would eventually cease to be maintained.

We have already seen what are the conditions which are favourable for the maintenance of life of the individual cell, no matter where situated. The principal condition is that it must be bathed by a nutrient fluid of suitable and constant composition. In higher animals this fluid is the lymph, which bathes the tissue elements and is itself constantly supplied with fresh nutriment and oxygen by the blood. Some tissue-cells are directly bathed by blood; and in invertebrates, in which there is no special system of lymph-vessels, all the tissues are thus nourished. All cells both take from and give to the blood, but not the same materials or to an equal extent. Some, such as the absorbing cells of the villi, almost exclusively give; others, such as the cells of the renal tubules, almost exclusively take. Nevertheless, the resultant of all the give and take throughout the body serves to maintain the composition of the blood constant under all circumstances. In this way the first condition of the maintenance of the life of the aggregate is fulfilled by insuring that the life of the individual cells composing it is kept normal.

The second essential condition for the maintenance of life of the cell-aggregate is the co-ordination of its parts and the due regulation of their activity, so that they may work together for the benefit of the whole. In the animal body this is effected in two ways: first, through the nervous system; and second, by the action of specific chemical substances which are formed in certain organs and carried by the blood to other parts of the body, the cells of which they excite to activity. These substances have received the general designation of 'hormones' (*ὁρμῶν*, to stir up), a term introduced by Professor Starling. Their action, and indeed their very existence, has only been recognised of late years, although the part which they play in the physiology of animals appears to be only second in importance to that of the nervous system itself; indeed, maintenance of life may become impossible in the absence of certain of these hormones.

Part played by the nervous system in the maintenance of aggregate life. Evolution of a nervous system. Before we consider the manner in which the nervous system serves to co-ordinate the life of the cell-aggregate, let us see how it has become evolved: The first step in the process was taken when certain of the cells of the external layer became specially sensitive to stimuli from outside, whether caused by mechanical impressions (tactile and auditory stimuli) or impressions of light and darkness (visual stimuli) or chemical impressions. The effects of such impressions were probably at first simply

communicated to adjacent cells and spread from cell to cell throughout the mass. An advance was made when the more impressionable cells threw out branching feelers amongst the other cells of the organism. Such feelers would convey the effects of stimuli with greater rapidity and directness to distant parts. They may at first have been retractile, in this respect resembling the long pseudopodia of certain Rhizopoda. When they became fixed they would be potential nerve-fibres and would represent the beginning of a nervous system. Even yet (as Ross Harrison has shown), in the course of development of nerve-fibres, each fibre makes its appearance as an amoeboid cell-process which is at first retractile, but gradually grows into the position it is eventually to occupy and in which it will become fixed.

In the further course of evolution a certain number of these specialised cells of the external layer sank below the general surface, partly perhaps for protection, partly for better nutrition: they became nerve-cells. They remained connected with the surface by a prolongation which became an afferent or sensory nerve-fibre, and through its termination between the cells of the general surface continued to receive the effects of external impressions; on the other hand, they continued to transmit these impressions to other, more distant cells by their efferent prolongations. In the further course of evolution the nervous system thus laid down became differentiated into distinct *afferent*, *efferent*, and *intermediary* portions. Once established, such a nervous system, however simple, must dominate the organism, since it would furnish a mechanism whereby the individual cells would work together more effectually for the mutual benefit of the whole.

It is the development of the nervous system, although not proceeding in all classes along exactly the same lines, which is the most prominent feature of the evolution of the Metazoa. By and through it all impressions reaching the organism from the outside are translated into contraction or some other form of cell-activity. Its formation has been the means of causing the complete divergence of the world of animals from the world of plants, none of which possess any trace of a nervous system. Plants react, it is true, to external impressions, and these impressions produce profound changes and even comparatively rapid and energetic movements in parts distant from the point of application of the stimulus—as in the well-known instance of the sensitive plant. But the impressions are in all cases propagated directly from cell to cell—not through the agency of nerve-fibres; and in the absence of anything corresponding to a nervous system it is not possible to suppose that any plant can ever acquire the least glimmer of intelligence. In animals, on the other hand, from a slight original modification of certain cells has directly proceeded in the course of evolution the elaborate structure of the nervous system with all its varied and complex func-

tions, which reach their culmination in the workings of the human intellect. 'What a piece of work is a man! How noble in reason! How infinite in faculty! In form and moving how express and admirable! In action how like an angel! In apprehension how like a god!' But lest he be elated with his psychological achievements, let him remember that they are but the result of the acquisition by a few cells in a remote ancestor of a slightly greater tendency to react to an external stimulus, so that these cells were brought into closer touch with the outer world; while on the other hand, by extending beyond the circumscribed area to which their neighbours remained restricted, they gradually acquired a dominating influence over the rest. These dominating cells became nerve-cells; and now not only furnish the means for transmission of impressions from one part of the organism to another, but in the progress of time have become the seat of perception and conscious sensation, of the formation and association of ideas, of memory, volition, and all the manifestations of the mind!

The most conspicuous part played by the nervous system in the phenomena of life is that which produces and regulates the general movements of the body—movements brought about by the so-called voluntary muscles. These movements are actually the result of impressions imparted to sensory or afferent nerves at the periphery—*e.g.*, in the skin or in the several organs of special sense; the effect of these impressions may not be immediate, but can be stored for an indefinite time in certain cells of the nervous system. The regulation of movements—whether they occur instantly after reception of the peripheral impression or result after a certain lapse of time; whether they are accompanied by conscious sensation or are of a purely reflex and unconscious character—is an intricate process, and the conditions of their co-ordination are of a complex nature involving not merely the causation of contraction of certain muscles, but also the prevention of contraction of others. For our present knowledge of these conditions we are largely indebted to the researches of Professor Sherrington.

A less conspicuous but no less important part played by the nervous system is that by which the contractions of involuntary muscles are regulated. Under normal circumstances these are always independent of consciousness, but their regulation is brought about in much the same way as is that of the contractions of voluntary muscles—*viz.*, as the result of impressions received at the periphery. These are transmitted by afferent fibres to the central nervous system, and from the latter other impulses are sent down, mostly along the nerves of the sympathetic or autonomic system of nerves, which either stimulate or prevent contraction of the involun-

tary muscles. Many involuntary muscles have a natural tendency to continuous or rhythmic contraction which is quite independent of the central nervous system; in this case the effect of impulses received from the latter is merely to increase or diminish the amount of such contraction. An example of this double effect is observed in connection with the heart, which—although it can contract regularly and rhythmically when cut off from the nervous system and even if removed from the body—is normally stimulated to increased activity by impulses coming from the central nervous system through the sympathetic, or to diminished activity by others coming through the vagus. It is due to the readiness by which the action of the heart is influenced in these opposite ways by the spread of impulses generated during the nerve-storms which we term ‘emotions’ that in the language of poetry, and even of every day, the word ‘heart’ has become synonymous with the emotions themselves.

Effects of emotions.

The involuntary muscle of the arteries has its action similarly balanced. When its contraction is increased, the size of the vessels is lessened and they deliver less blood; the parts they supply accordingly become pale in colour. On the other hand, when the contraction is diminished the vessels enlarge and deliver more blood; the parts which they supply become correspondingly ruddy. These changes in the arteries, like the effects upon the heart, may also be produced under the influence of emotions: Thus ‘blushing’ is a purely physiological phenomenon due to diminished action of the muscular tissue of the arteries, whilst the pallor produced by fright is caused by an increased contraction of that tissue. Apart, however, from these conspicuous effects, there is constantly proceeding a less apparent but not less important balancing action between the two sets of nerve-fibres distributed to heart and blood-vessels; which are influenced in one direction or another by every sensation which we experience and even by impressions of which we may be wholly unconscious, such as those which occur during sleep or anæsthesia, or which affect our otherwise insensitive internal organs.

A further instance of nerve-regulation is seen in secreting glands. Not all glands are thus regulated, at least not directly; but in those which are, the effects are striking. Their regulation is of

Regulation of secretion by the nervous system.

the same general nature as that exercised upon involuntary muscle, but it influences the chemical activities of the gland-cells and the outpouring of secretion from them. By means of this regulation a secretion can be produced or arrested, increased or diminished. As with muscle, a suitable balance is in this way maintained, and the activity of the glands is adapted to the requirements of the organism. Most of the digestive glands are

thus influenced, as are the skin-glands which secrete sweat. And by the action of the nervous system upon the skin-glands, together with its effect in increasing or diminishing the blood-supply to the cutaneous blood-vessels, the temperature of our blood is regulated and is kept at the point best suited for maintenance of the life and activity of the tissues.

**Regulation of
body tem-
perature.**

The action of the nervous system upon the secretion of glands is strikingly exemplified, as in the case of its action upon the heart and blood-vessels by the effects of the emotions. Thus an emotion of one kind—such as the anticipation of food—will cause saliva to flow—‘the mouth to water’; whereas an emotion of another kind—such as fear or anxiety—will stop the secretion, causing the ‘tongue to cleave unto the roof of the mouth,’ and rendering speech difficult or impossible. Such arrest of the salivary secretion also makes the swallowing of dry food difficult: advantage of this fact is taken in the ‘ordeal by rice’ which used to be employed in the East for the detection of criminals.

**Effects of
emotions on
secretion.**

The activities of the cells constituting our bodies are controlled, as already mentioned, in another way than through the nervous system, viz., by chemical agents (hormones) circulating in the blood. Many of these are produced by special glandular organs, known as internally secreting glands. The ordinary secreting glands pour their secretions on the exterior of the body or on a surface communicating with the exterior; the internally secreting glands pass the materials which they produce directly into the blood. In

**Regulation by
chemical
agents :
hormones.
Internal
secretions.**

this fluid the hormones are carried to distant organs. Their influence upon an organ may be essential to the proper performance of its functions or may be merely ancillary to it. In the former case removal of the internally secreting gland which produces the hormone, or its destruction by disease, may prove fatal to the organism. This is the case with the suprarenal capsules: small glands which are adjacent to the kidneys, although having no physiological connection with these organs. A Guy's physician, Dr. Addison, in the middle of the last century showed that a certain affection, almost always fatal, since known by his name, is associated with disease of the suprarenal capsules. A short time after this observation a French physiologist, Brown-Séquard, found that animals from which the suprarenal capsules are removed rarely survive the operation for more than a few days. In the concluding decade of the last century interest in these bodies was revived by the discovery that they are constantly yielding to the blood a chemical agent (or hormone) which stimulates the contractions of the heart and arteries and assists in the promotion of every action which is brought about through the sympathetic nervous

Suprarenals.

system (Langley). In this manner the importance of their integrity has been explained, although we have still much to learn regarding their functions.

Another instance of an internally secreting gland which is essential to life, or at least to its maintenance in a normal condition, is the thyroid. The association of imperfect development or

Thyroid.

disease of the thyroid with disorders of nutrition and inactivity of the nervous system is well ascertained. The form of idiocy known as cretinism and the affection termed myxœdema are both associated with deficiency of its secretion: somewhat similar conditions to these are produced by the surgical removal of the gland. The symptoms are alleviated or cured by the administration of its juice. On the other hand, enlargement of the thyroid, accompanied by increase of its secretion, produces symptoms of nervous excitation, and similar symptoms are caused by excessive administration of the glandular substance by the mouth. From these observations it is inferred that the juice contains hormones which help to regulate the nutrition of the body and serve to stimulate the nervous system, for the higher functions of which they appear to be essential. To quote M. Gley, to whose researches we owe much of our knowledge regarding the functions of this organ: 'La genèse et l'exercice des plus hautes facultés de l'homme sont conditionnés par l'action purement chimique d'un produit de sécrétion. Que les psychologues méditent ces faits!'

The case of the parathyroid glandules is still more remarkable. These organs were discovered by Sandström in 1880. They are four

Parathyroids. minute bodies, each no larger than a pin's head, imbedded in the thyroid. Small as they are, their internal

secretion possesses hormones which exert a powerful influence upon the nervous system. If they are completely removed, a complex of symptoms, technically known as 'tetany,' is liable to occur, which is always serious and may be fatal. Like the hormones of the thyroid itself, therefore, those of the parathyroids produce effects upon the nervous system, to which they are carried by the blood; although the effects are of a different kind.

Another internally secreting gland which has evoked considerable interest during the last few years is the pituitary body. This is a small

Pituitary. structure no larger than a cob-nut attached to the base of the brain. It is mainly composed of glandular cells. Its

removal has been found (by most observers) to be fatal—often within two or three days. Its hypertrophy, when occurring during the general growth of the body, is attended by an undue development of the skeleton, so that the stature tends to assume gigantic proportions. When the hypertrophy occurs after growth is completed, the extremities—viz., the hands and feet, and the bones of the face—are mainly affected; hence

the condition has been termed 'acromegaly' (enlargement of extremities). The association of this condition with affections of the pituitary was pointed out in 1885 by a distinguished French physician, Dr. Pierre Marie. Both 'giants' and 'acromegalists' are almost invariably found to have an enlarged pituitary. The enlargement is generally confined to one part—the anterior lobe—and we conclude that this produces hormones which stimulate the growth of the body generally and of the skeleton in particular. The remainder of the pituitary is different in structure from the anterior lobe and has a different function. From it hormones can be extracted which, like those of the suprarenal capsule, although not exactly in the same manner, influence the contraction of the heart and arteries. Its extracts are also instrumental in promoting the secretion of certain glands. When injected into the blood they cause a free secretion of water from the kidneys and of milk from the mammary glands, neither of which organs are directly influenced (as most other glands are) through the nervous system. Doubtless under natural conditions these organs are stimulated to activity by hormones which are produced in the pituitary and which pass from this into the blood.

The internally secreting glands which have been mentioned (thyroid, parathyroid, suprarenal, pituitary) have, so far as is known, no other function than that of producing chemical substances of this character for the influencing of other organs, to which they are conveyed by the blood. It is interesting to observe that these glands are all of very small size, none being larger than a walnut, and some—the parathyroids—almost microscopic. In spite of this, they are essential to the proper maintenance of the life of the body, and the total removal of any of them by disease or operation is in most cases speedily fatal.

There are, however, organs in the body yielding internal secretions to the blood in the shape of hormones, but exercising at the same time other functions. A striking instance is furnished by the **Pancreas**. The pancreas, the secretion of which is the most important of the digestive juices. This—the pancreatic juice—forms the external secretion of the gland, and is poured into the intestine, where its action upon the food as it passes out from the stomach has long been recognised. It was, however, discovered in 1889 by von Mering and Minkowski that the pancreas also furnishes an internal secretion, containing a hormone which is passed from the pancreas into the blood, by which it is carried first to the liver and afterwards to the body generally. This hormone is essential to the proper utilisation of carbohydrates in the organism. It is well known that the carbohydrates of the food are converted into grape sugar and circulate in this form in the blood, which always contains a certain amount; the blood conveys it to all the cells of the body, and they utilise it as fuel. If, owing to disease of the pan-

creas or as the result of its removal by surgical procedure, its internal secretion is not available, sugar is no longer properly utilised by the cells of the body and tends to accumulate in the blood; from the blood the excess passes off by the kidneys, producing diabetes.

Another instance of an internal secretion furnished by an organ which is devoted largely to other functions is the 'pro-secretin' found in the cells lining the duodenum. When the acid gastric juice comes into contact with these cells it converts their pro-secretin into 'secretin.' This is a hormone which is passed into the blood and circulates with that fluid. It has a specific effect on the externally secreting cells of the pancreas, and causes the rapid outpouring of pancreatic juice into the intestine. This effect is similar to that of the hormones of the pituitary body upon the cells of the kidney and mammary gland. It was discovered by Bayliss and Starling.

The reproductive glands furnish in many respects the most interesting example of organs which—besides their ordinary products, the germ- and sperm-cells (ova and spermatozoa)—form hormones which circulate in the blood and effect changes in cells of distant parts of the body. It is through these hormones that the secondary sexual characters, such as the comb and tail of the cock, the mane of the lion, the horns of the stag, the beard and enlarged larynx of a man, are produced, as well as the many differences in form and structure of the body which are characteristic of the sexes. The dependence of these so-called secondary sexual characters upon the state of development of the reproductive organs has been recognised from time immemorial, but has usually been ascribed to influences produced through the nervous system, and it is only in recent years that the changes have been shown to be brought about by the agency of internal secretions and hormones, passed from the reproductive glands into the circulating blood.²⁶

It has been possible in only one or two instances to prepare and isolate the hormones of the internal secretions in a sufficient condition of purity to subject them to analysis, but enough is known about them to indicate that they are organic bodies of a not very complex nature, far simpler than proteins and even than enzymes. Those which have been studied are all dialysable, are readily soluble in water but insoluble in alcohol, and are not destroyed by boiling. One at least—that of the medulla of the suprarenal capsule—has been prepared synthetically, and when their

²⁶ The evidence is to be found in F. H. A. Marshall, *The Physiology of Reproduction*, 1911.

exact chemical nature has been somewhat better elucidated it will probably not be difficult to obtain others in the same way.

From the above it is clear that not only is a co-ordination through the nervous system necessary in order that life shall be maintained in a normal condition, but a chemical co-ordination is no less essential. These may be independent of one another; but on the other hand they may react upon one another. For it can be shown that the production of some at least of the hormones is under the influence of the nervous system (Biedl, Asher, Elliott); whilst, as we have seen, some of the functions of the nervous system are dependent upon hormones.

Time will not permit me to refer in any but the briefest manner to the protective mechanisms which the cell aggregate has evolved for its defence against disease, especially disease produced by parasitic micro-organisms. These, which belong with few exceptions to the Protista, are without doubt the most formidable enemies which the multicellular Metazoa, to which all the higher animal organisms belong, have to contend against. To such micro-organisms are due *inter alia* all diseases which are liable to become epidemic, such as anthrax and rinderpest in cattle, distemper in dogs and cats, small-pox, scarlet fever, measles, and sleeping sickness in man. The advances of modern medicine have shown that the symptoms of these diseases—the disturbances of nutrition, the temperature, the lassitude or excitement, and other nervous disturbances—are the effects of chemical poisons (*toxins*) produced by the micro-organisms and acting deleteriously upon the tissues of the body. The tissues, on the other hand, endeavour to counteract these effects by producing other chemical substances destructive to the micro-organisms or antagonistic to their action: these are known as *anti-bodies*. Sometimes the protection takes the form of a subtle alteration in the living substance of the cells which renders them for a long time, or even permanently, insusceptible (immune) to the action of the poison. Sometimes certain cells of the body, such as the white corpuscles of the blood, eat the invading micro-organisms and destroy them bodily by the action of chemical agents within their protoplasm. The result of an illness thus depends upon the result of the struggle between these opposing forces—the micro-organisms on the one hand and the cells of the body on the other—both of which fight with chemical weapons. If the cells of the body do not succeed in destroying the invading organisms it is certain that the invaders will in the long run destroy them, for in this combat no quarter is given. Fortunately we have been able, by the aid of animal experimentation, to acquire some knowledge of the manner in which we are attacked by micro-organisms and of the methods which the cells of our body

**Protective
chemical
mechanisms.
Toxins and
antitoxins.**

adopt to repel the attack, and the knowledge is now extensively utilised to assist our defence. For this purpose protective serums or anti-toxins, which have been formed in the blood of other animals, are employed to supplement the action of those which our own cells produce. It is not too much to assert that the knowledge of the parasitic origin of so many diseases and of the chemical agents which

**Parasitic
nature of
diseases.**

on the one hand cause, and on the other combat, their symptoms, has transformed medicine from a mere art practised empirically, into a real science based upon experiment. The transformation has opened out an illimitable vista of possibilities in the direction not only of cure, but, more important still, of prevention. It has taken place within the memory of most of us who are here present. And only last February the world was mourning the death of one of the greatest of its benefactors—a former President of this Association²⁷—who, by applying this knowledge to the practice of surgery, was instrumental, even in his own lifetime, in saving more lives than were destroyed in all the bloody wars of the nineteenth century!

The question has been debated whether, if all accidental modes of destruction of the life of the cell could be eliminated, there would

**Senescence
and death.**

remain a possibility of individual cell-life, and even of aggregate cell-life, continuing indefinitely; in other words, Are the phenomena of senescence and death a natural and necessary sequence to the existence of life? To most of my audience it will appear that the subject is not open to debate. But some physiologists (*e.g.*, Metchnikoff) hold that the condition of senescence is itself abnormal; that old age is a form of disease or is due to disease, and, theoretically at least, is capable of being eliminated. We have already seen that individual cell-life, such as that of the white blood-corpuscles and of the cells of many tissues, can under suitable conditions be prolonged for days or weeks or months after general death. Unicellular organisms kept under suitable conditions of nutrition have been observed to carry on their functions normally for prolonged periods and to show no degeneration such as would accompany senescence. They give rise by division to others of the same kind, which also, under favourable conditions, continue to live, to all appearance indefinitely. But these instances, although they indicate that in the simplest forms of organisation existence may be greatly extended without signs of decay, do not furnish conclusive evidence of indefinite prolongation of life. Most of the cells which constitute the body, after a period of growth and activity, sometimes more, sometimes less prolonged, eventually undergo atrophy and cease to perform satisfactorily the

²⁷ Lord Lister was President at Liverpool in 1896.

functions which are allotted to them. And when we consider the body as a whole, we find that in every case the life of the aggregate consists of a definite cycle of changes which, after passing through the stages of growth and maturity, always leads to senescence, and finally terminates in death. The only exception is in the reproductive cells, in which the processes of maturation and fertilisation result in rejuvenescence, so that instead of the usual downward change towards senescence, the fertilised ovum obtains a new lease of life, which is carried on into the new-formed organism. The latter again itself ultimately forms reproductive cells, and thus the life of the species is continued. It is only in the sense of its propagation in this way from one generation to another that we can speak of the indefinite continuance of life: we can only be immortal through our descendants!

The individuals of every species of animal appear to have an average duration of existence.²⁸ Some species are known the individuals of which live only for a few hours, whilst others survive for a hundred years.²⁹ In man himself the average length of life would probably be greater than the three-score and ten years allotted to him by the Psalmist if we could eliminate the results of disease and accident; when these results are included it falls far short of that period. If the terms of life given in the purely mythological part of the Old Testament were credible, man would in the early stages of his history have possessed a remarkable power of resisting age and disease. But, although many here present were brought up to believe in their literal veracity, such records are no longer accepted even by the most orthodox of theologians, and the nine hundred odd years with which Adam and his immediate descendants are credited, culminating in the nine hundred and sixty-nine of Methuselah, have been relegated, with the account of Creation and the Deluge, to their proper position in literature. When we come to the Hebrew Patriarchs, we notice a considerable diminution to have taken place in what the insurance offices term the 'expectation of life.' Abraham is described as having lived only to 175 years, Joseph and Joshua to 110, Moses to 120; even at that age 'his eye was not dim nor his natural force abated.' We cannot say that under ideal conditions all these terms are impossible; indeed, Metchnikoff is disposed to regard them as probable; for great ages are still occasionally recorded, although it is doubtful if any as considerable as these are ever substantiated. That the expectation of life was

²⁸ This was regarded by Buffon as related to the period of growth, but the ratio is certainly not constant. The subject is discussed by Ray Lankester in an early work: *On Comparative Longevity in Man and Animals*, 1870.

²⁹ The approximately regular periods of longevity of different species of animals furnishes a strong argument against the theory that the decay of old age is an accidental phenomenon, comparable with disease.

better than than now would be inferred from the apologetic tone adopted by Jacob when questioned by Pharaoh as to his age: 'The days of the years of my pilgrimage are a hundred and thirty years; few and evil have the days of the years of my life been, and have not attained unto the days of the years of the life of my fathers in the days of their pilgrimage.' David, to whom, before the advent of the modern statistician, we owe the idea that seventy years is to be regarded as the normal period of life,³⁰ is himself merely stated to have 'died in a good old age.' The periods recorded for the Kings show a considerable falling-off as compared with the Patriarchs; but not a few were cut off by violent deaths, and many lived lives which were not ideal. Amongst eminent Greeks and Romans few very long lives are recorded, and the same is true of historical persons in mediæval and modern history. It is a long life that lasts much beyond eighty; three such linked together carry us far back into history. Mankind is in this respect more favoured than most mammals, although a few of these surpass the period of man's existence.³¹ Strange that the brevity of human life should be a favourite theme of preacher and poet when the actual term of his 'erring pilgrimage' is greater than that of most of his fellow creatures!

The modern applications of the principles of preventive medicine and hygiene are no doubt operating to lengthen the average life. But even if the ravages of disease could be altogether eliminated, it is certain that at any rate the fixed cells of our body must eventually grow old and ultimately cease to function; when this happens to cells which are essential to the life of the organism, general death must result. This will always remain the universal law, from which there is no escape. 'All that lives must die, passing through nature to eternity.'

Such natural death unaccelerated by disease—is not death by disease as unnatural as death by accident?—should be a quiet, painless phenomenon, unattended by violent change. As Dastre expresses it, 'The need of death should appear at the end of life, just as the need of sleep appears at the end of the day.' The change has been led gradually up to by an orderly succession of phases, and is itself the last manifestation of life. Were we all certain of a quiet passing—were we sure that there would be 'no moaning of the bar when we go out to sea'—we could anticipate the coming of death after a ripe old age without apprehension. And if ever the time shall arrive when man will have learned to regard this change as a simple physiological process, as natural as

³⁰ The expectation of life of a healthy man of fifty is still reckoned at about twenty years.

³¹ *Hominis ævum cæterorum animalium omnium superat præter admodum paucorum.*—Francis Bacon, *Historia vitæ et mortis*, 1637.

the oncoming of sleep, the approach of the fatal shears will be as generally welcomed as it is now abhorred. Such a day is still distant; we can hardly say that its dawning is visible. Let us at least hope that, in the manner depicted by Dürer in his well-known etching, the sunshine which science irradiates may eventually put to flight the melancholy which hovers, bat-like, over the termination of our lives, and which even the anticipation of a future happier existence has not hitherto succeeded in dispersing. .

